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PREPARATORY SET (EXPECTANCY)—AN EXPERIMENTAL DEMONSTRATION OF ITS 'CENTRAL' LOCUS

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INTRODUCTION

Any intimation that psychologically significant events can occur in the human nervous system without concomitant manifestations in 'motor' mechanisms (muscles or glands) is still tantamount in many quarters to a renunciation of scientific objectivism and a return to the presuppositions of theology and philosophy. With the abandonment of the traditional methodologies of speculation and introspection, psychologists, especially in America, pinned their hope to the Watsonian doctrine that all mental processes involve peripheral stimulation and response, implicit or overt. During the past two decades, strenuous efforts have been made to justify this supposition, but the results have been equivocal: sometimes a given psychological process is accompanied by detectable motor phenomena and sometimes not. Failure to demonstrate the sought-for concomitance has commonly prompted comment to the effect that the technique was inadequate or that the experiment was not properly conceived. May it not be time to take stock of the situation realistically and make sure that we are not pursuing a will-o'-the-wisp?

Presumably the only scientific grounds for wishing to make all psychological processes dependent upon concomitant

stimulus-response sequences was the supposition that this would facilitate their empirical investigation. This supposition does not appear to have been justified. But in abandoning it one does not have to give up the objective approach to psychological problems. In a recent study (**8**) the senior author has proposed a number of ways in which at least one important psychological phenomenon can be fruitfully investigated with acceptable objectivity yet without recourse to the hypothesis that mental events necessarily involve reactions outside the central nervous system. The present paper is supplementary to the one cited and purports to give additional support for the position here taken.

Problem

Equating 'attention' and 'preparatory set,' a number of writers have undertaken to show that in the final analysis this phenomenon depends upon the assumption and maintenance of characteristic body postures and patterns of muscle tension.[1] In many instances, where expectation of a given stimulus and preparation for appropriate action are very highly developed, peripheral adjustments are indeed conspicuous. Optimally quick, efficient reactions frequently involve preliminary bodily orientation of a particular kind. One cannot, for example, leap into the air without first crouching. But in other instances of readiness for action of a particular kind, motor accompaniments are apparently lacking. One can, presumably, adopt and maintain a particular posture without necessarily being prepared for a given stimulus-response sequence, and one can likewise be so prepared without necessarily giving postural indications of this. These considerations suggest that posturing and muscle tensing are probably accessory rather than intrinsic aspects of the attentional process. The present investigation represents an attempt to determine whether preparatory set (expectancy, attention) is or is not invariably reflected in motor accompaniments. It is based upon the three following predictions,

[1] *Cf.* reviews by Dashiell (**1**), Freeman (**2**), Mowrer (**9**), Woodworth (**10**), and Young (**12**).

which the experimental procedure described below was designed to test in the order in which these predictions are here listed:

I. *If human subjects are instructed to make the same, identical response (and that as quickly as possible) to either of two radically different stimuli whenever either type of stimulus is presented ('divided attention'), they will show a longer latency of response to an uninterrupted series of presentations of only one of these stimuli than will subjects who have been instructed to expect and react to only the type of stimulus actually presented ('unified attention').*

II. *If human subjects are instructed to make the same, identical response to either of two radically different stimuli whenever either type of stimulus is presented, their response latencies will materially increase if an uninterrupted series of one type of stimulus is followed (unexpectedly) by the other type of stimulus.*

III. *If human subjects are instructed to make the same, identical response to either of two radically different stimuli whenever either type of stimulus is presented, their response latencies will materially increase if an expected change from one type of stimulus to the other fails to occur.*

These three predictions are based upon the assumption that if preparatory set is primarily dependent upon the position of and muscular tensions prevailing in the bodily member (or members) involved in the prepared-for reaction, this reaction should occur, under the conditions stated, with equal promptness regardless of which type of stimulus is used to elicit it. If, on the other hand, preparatory set is primarily a central phenomenon, greater expectation of one type of stimulus should, by virtue of the temporarily altered neural connections underlying this phenomenon, result in a lowering of the latency of response to the more expected stimulus and in an increase in the latency of response to the less expected stimulus. It can scarcely be supposed that the muscular and postural preparation for making exactly the same type of response to two different stimuli is different for the two stimuli. If differences in the latency of such a reaction to these two

stimuli are found to occur with changes in expectation, if, in short, the three predictions made above are confirmed, this finding will support the hypothesis that preparatory set is primarily a 'central' rather than a motor phenomenon.

Procedure

Thirty-nine male college students were employed as subjects in this investigation and were divided randomly into two groups. Group I contained 18 subjects and Group II contained 21 subjects.[2] The procedure employed with Group I was as follows. Each subject was taken into a sound-proof room and seated in a comfortable chair, on the right arm of which was a telegraph key that could be conveniently operated. The subject was then instructed to hold this key down with a gentle but constant pressure, releasing it only when a three-watt neon lamp, located five feet directly in front of him at the level of his eyes, flashed on or when a tone (43 d.b. above threshold, 1,000 d.v. pitch) sounded in a pair of headphones, located three feet in front and to the right of the subject. It was explained that the reaction stimulus (light, tone) would remain on until the subject released the telegraph key and that the latencies of his reactions would be accurately timed and recorded. An incentive for responding as quickly as possible was provided by announcing that upon completing the experiment (which would require about 10 minutes), each subject would be told how his quickness of response compared with that of other persons under similar circumstances. It was made clear to each subject that after making a response, he was to push the key down again within a second or two and wait attentively for the next stimulus. Upon leaving the sound-proof room, the experimenter extinguished the illumination, with an explanatory remark to the effect that this was done in order that the subject's attentiveness would not be impaired by irrelevant visual stimuli.

After each Group-I subject was properly installed in the experimental situation, he received 10 tones at *regular six-second intervals*. At the end of this series, the experimenter entered the sound-proof room and told the subject that he should now dismiss the possibility of the light occurring as a stimulus and concentrate only upon the series of tones that was to ensue. The subject then received a second series of 10 tones, identical with the first series. At the end of the second series, the experimenter again entered the sound-proof room and told the subject to resume his former attitude of readiness to respond to either tone or light and to maintain this attitude until the end of the experiment. Lights and tones were then presented in the following order: 7 tones, 9 lights, 13 tones, 21 lights, and then 11 tones and 10 lights in simple alternation, except for the fact that between the 9th and 10th lights, two tones were presented in succession. The second of these tones occurred at the standard interval of six seconds after the preceding tone, *i.e.*, at a point in time at which a light would have been expected in a simple alternation schedule.

The procedure for Group II differed from that employed with Group I in only one respect. Instead of being told at the beginning of the experiment to be prepared to react to both lights and tones, each of the 21 subjects in Group II was first told that he would receive and was to react as indicated above to a series of tones, with no mention

[2] The difference in the number of subjects in the two groups was due to an inadvertence, not design. Ideally, the two groups should have been equally large, but in this particular investigation the slight inequality probably did not significantly affect the results obtained.

of any form of visual stimulation.) (After 10 tones had been presented under these conditions, at regular six-second intervals, the experimenter entered the sound-proof room and told the subject that he should continue to react to the tone whenever it occurred but that in addition he should now also be prepared to react, in exactly the same manner, to the neon lamp (the location of which was indicated) whenever it flashed on.) The experiment was then resumed, with the subject receiving tones and lights in the following order: 17 tones, 9 lights, 13 tones, 21 lights, and 11 tones and 10 lights in simple alternation, except for the fact that between the 9th and 10th lights, two tones were presented in succession, as explained in the preceding paragraph.

It will be noted that the subjects in Group I and in Group II received the same number of tones (51 in all) and the same number of lights (40 in all) and in exactly the same order: 27 tones, 9 lights, 13 tones, 21 lights, and 11 tones and 10 lights in the order already indicated. The only difference was in the instructions given to the two groups. During the presentation of the first 10 tones, the subjects in Group I were instructed to be prepared to react to both lights and tones, whereas the subjects in Group II were instructed to be prepared to react only to tones. During the presentation of the second series of 10 tones, the conditions were reversed: the subjects in Group I were instructed to be prepared to react only to tones, whereas the subjects in Group II were instructed to be prepared to react to both lights and tones. From the end of the second series of 10 tones, *i.e.*, from the 21st stimulus on, the subjects in both groups worked under identical instructions, namely, to be prepared to react to both lights and tones.

Results

The solid-line curve in Fig. 1 shows the average reaction times, in milliseconds, of all 39 subjects to 10 successive tones (first 10 in case of Group I, second 10 in case of Group II), with subjects acting under instructions to react to both tones and lights. The dotted-line curve in Fig. 1 shows, on the other hand, the average reaction times, in milliseconds, of all 39 subjects to 10 successive tones (second 10 in case of Group I, first 10 in case of Group II), when acting under instructions to react only to tones. As will be noted, the average reaction times were consistently longer under the one set of instructions than under the other.

The results presented in Fig. 1 fully confirm Prediction I as stated above.

The average reaction times of all 39 subjects to the successive series of tones and lights which followed the first and second series of tone referred to immediately above are presented in Fig. 2. The filled-in circles represent auditory reaction times, and the open circles represent visual reaction times. It will be observed that whenever a change occurred in the nature of the reaction stimulus, the average reaction

times always increased markedly, thereafter quickly settling to a fairly even baseline. (It will also be noted, incidentally, that the baseline for visual reaction time is somewhat higher

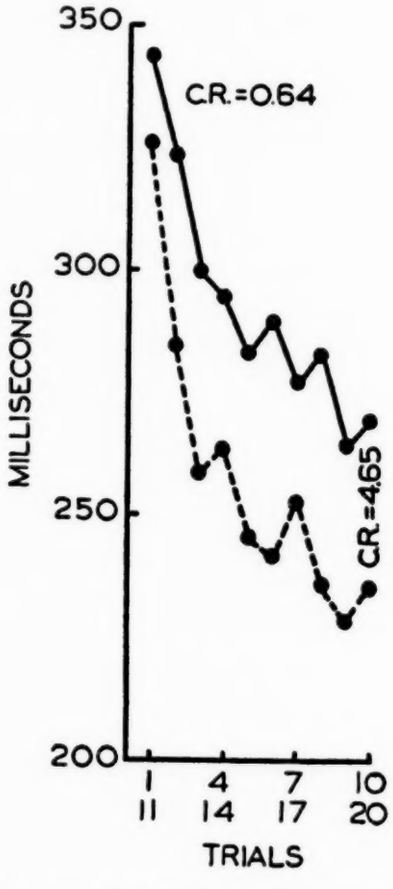

FIG. 1. Successive latencies of a simple response to a recurrent auditory stimulus (at six-second intervals) under two conditions: (*a*) subjects prepared, by instructions, to react only to auditory stimulation—dotted line; and (*b*) subjects prepared to react both to auditory and to visual stimulation—solid line.

than that for auditory reaction time, but this fact is of no immediate importance.)

The results presented in Fig. 2 thus confirm Prediction II as stated above.

In Fig. 3 the filled-in circles represent the average auditory reaction times of all 39 subjects and the open circles represent their average visual reaction times, with tones and lights succeeding each other in the order indicated. In making Prediction III, it was reasoned that if an increase in reaction time could be produced by changing from one type of stimulus to another when such a change is not expected (Prediction II),

a similar increase in reaction time could be produced by failing to make such a change when it is expected.) The most direct way of testing this assumption seemed to involve the presentation of a number of tones and lights in simple alternation, followed, let us say, by two tones in succession, where a

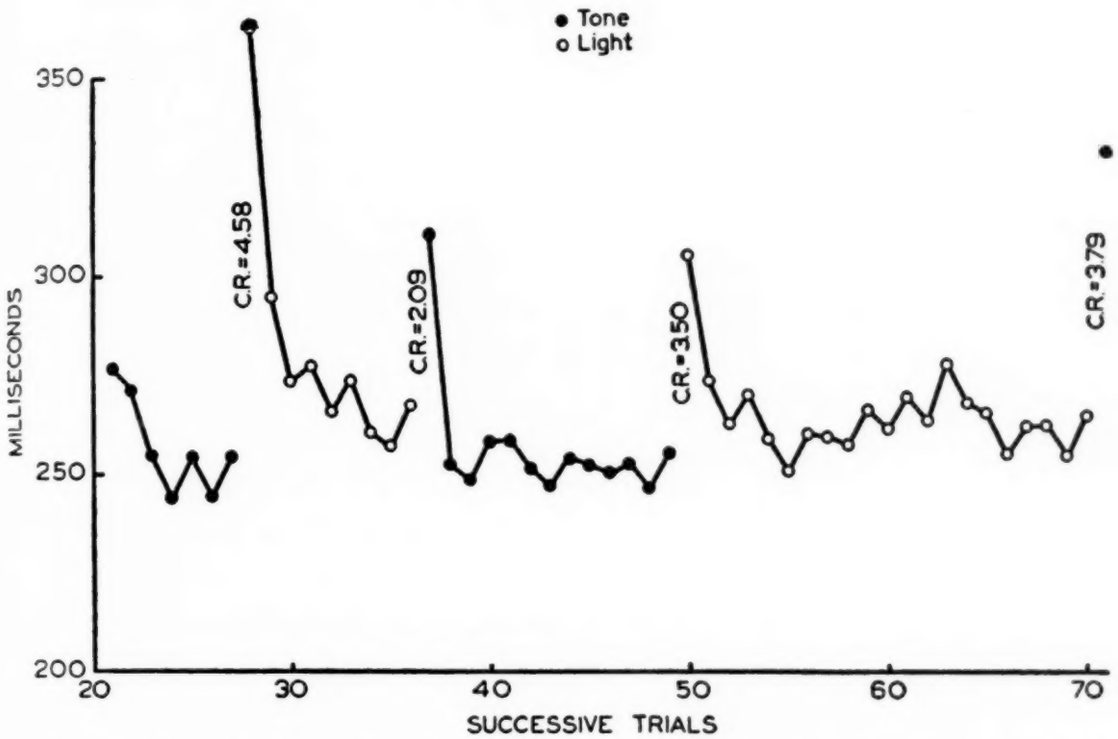

FIG. 2. Successive latencies of a simple response to alternating irregular series of auditory and visual stimuli, with subjects instructed to react to both.

tone and a light would be the normal expectation. This reasoning accounts for the particular temporal order of the last 21 tones and lights presented in the present experiment. On the basis of Prediction III, the average reaction time to a tone presented in this series at a point in time at which a light would seem most probable should be significantly longer than the reaction time to the preceding tone.

The results presented in Fig. 3 do not confirm Prediction III. According to this prediction the average latency of response to the tone presented on the 90th trial should have been significantly longer than the average latency of response to the tone presented on the 89th trial. As Fig. 3 shows, such was not the case.

Discussion

The results presented in Fig. 1 so completely confirm the first of the three predictions made above that little additional comment is required on this score. With subjects prepared,

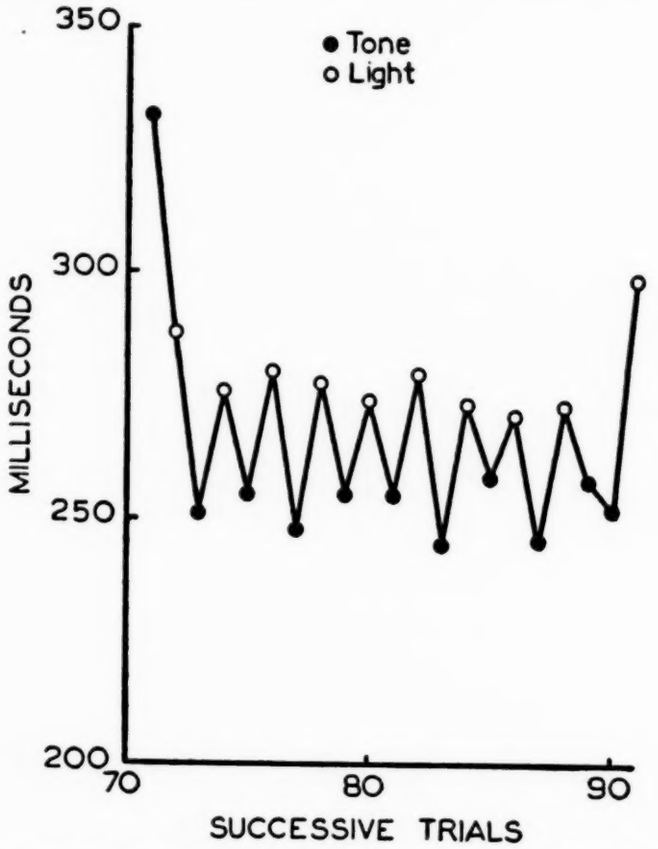

Fig. 3. Successive latencies of a simple response to auditory and visual stimuli presented in simple alternation, save for the two auditory stimuli occurring together near end of series.

by instructions, to react to both lights and tones, their reaction times to a series of tones were consistently longer than when the same subjects were prepared to react to tones only. As already emphasized, the type of response made in both cases was exactly the same. If preparatory set (expectancy, attention) is primarily a function of the position and tonicity of the responding member, readiness to respond to tone should have involved equal readiness to respond to light (or, in fact, any other stimulus) and *vice versa*. It should, therefore, have made no difference in the latency of such a response whether the subjects were instructed to be prepared to respond to both tones and light or only to tones. The

fact that response latency, which is here taken to be a valid measure of readiness or set, varies significantly under the two conditions stated is thus interpreted as meaning that set (expectancy, attention) is not primarily a function of the position and tonicity of the responding bodily member or members. It is inferred to be instead primarily a function of neural, or 'central,' factors.

One of the most striking secondary characteristics of the two curves presented in Fig. 1 is the fact that they are relatively close together in the beginning (at their origin near the ordinate) and become progressively more widely separated. The present tentative hypothesis offered to account for this progressive divergence is that the downward trend of both curves was due to what may be termed 'practice effect' but that in the case of the upper curve this trend was retarded somewhat by the antagonistic influence of a second factor. With subjects prepared by instructions to react to tones only, expectation of (readiness for) this type of stimulation would presumably increase with each successive presentation of tone. In other words, the expectation established by the instructions would be strengthened by the actual experimental procedure, with a resulting increase in the speed of the ensuing reactions. If, on the other hand, subjects were told to be prepared to react to both lights and tones, such instructions would carry the implication that both tones and lights were going to occur. Under these circumstances, expectation of each type of stimulation would presumably be equally strong in the beginning, but with succeeding presentations of only one type of stimulus, namely, tone, a change would necessarily occur in the set or attitude established by the instructions.

If a group of human subjects are asked to predict how a tossed coin is going to fall, on the first trial about half of them will predict that the coin will land 'heads' up and about half 'tails' up.[3] If, now, the coin should happen to fall 'tails' on the first two or three tosses, the subjects will show an increasing tendency to predict 'heads' on the ensuing trials. If the coin falls 'tails' a few more times, all subjects are likely

[3] Under some conditions there is actually a greater tendency to predict 'heads' on the first trial (3), but for purposes of the present discussion this fact can be disregarded.

soon to be predicting that on the next toss the coin will land 'heads' up.[4] If, however, the coin continues indefinitely to land 'tails' up, the subjects will eventually cease predicting 'heads' and will instead predict only 'tails.' They will, in short, abandon the hypothesis that the coin is behaving according to the laws of chance and adopt instead the hypothesis that the coin is 'loaded' or otherwise so controlled that it always falls in the same manner.

Apparently something of this kind occurred when the subjects in the present experiment were prepared by instructions to react to both tones and lights but were presented with only tones. With each successive presentation of tone, expectation of light conjecturally mounted higher and higher. If this occurred it would account for the fact that the upper curve in Fig. 1 descends so slowly, in comparison with the lower curve. With increasing expectation of light, readiness to react to tone would diminish, with consequent prolongation of the latency of response to the latter type of stimulus. Had it not been for what is here termed 'practice effect,'[5] the upper curve in Fig. 1 would presumably have actually risen rather than fallen.

The validity of the foregoing explanation can be readily tested in the following fashion. If one group of subjects were instructed to be prepared to react to both tones and lights in the manner described above and were then presented with a series, let us say, of 30 or 40 tones exclusively, and if a second group of subjects were instructed to be prepared to react to tones only, the curves obtained by plotting the average reaction times of the two groups would at first presumably diverge, just as the two curves shown in Fig. 1 do, but eventually the curve for the first group would descend and

[4] This tendency to be swayed in one's prediction of future chance events by the nature of past chance events has been called 'gambler's fallacy.' Logically it is indeed a fallacy, but it shows the primordial force of immediate experience in shaping behavior. Moreover, in many situations there is always the possibility that a given type of event which has previously behaved in a chance manner will not do so in the future. 'Gambler's fallacy' thus has a potential adaptional value.

[5] A theoretical basis for this effect will be given in a later paper.

coincide with the curve for the second group.[6] This would occur at the point at which the subjects in the first group abandoned the 'light-tone-hypothesis' established by the instructions and adopted, on the basis of experience, a simple 'tone-hypothesis.' The results of such an experiment will soon be reported.

The results presented in Fig. 2 also confirm so fully the second of the three predictions made at the outset of this paper that there is here again need for relatively little discussion. These results clearly support the view that preparatory set, as measured by the rapidity of the response for which the set is appropriate, involves neural factors which are capable of varying quite independently of posture and muscular tonicity. There is apparently no other basis for explaining the dramatic way in which the latency of the response under investigation increased when an uninterrupted series of the one type of stimulus was succeeded by the other type of stimulus.

It may, of course, be suggested that this effect was correlated with variations in the motor adjustments of the respective sense organs, but this possibility is very remote. Hall and von Kries (4) demonstrated more than half a century ago that the latency of a simple reaction to a standard visual stimulus presented in the periphery of the retina may be as much as 44 percent greater than the latency of reaction to the same stimulus presented in the forvea. On the assumption that during a long series of tones the subjects in the present investigation allowed their eyes to wander widely from the point in space at which the neon lamp was located, it could be reasoned that this variation in sense-organ adjustment caused the delay in speed of reaction on the first presentation of the ensuing visual stimuli. However, since movement of the ears could not be made similarly to account for the increased latency of the reactions to the first of a series of tones following a series of lights, such a theory has little cogency.

[6] For a discussion of the role of 'hypothesis' formation, based upon habits of estimating probabilities, in determining behavior in another type of experimental situation see articles by Humphreys (6) and by Humphreys, Miller and Ellson (7). See also Goodfellow (3).

Neither can the well-known difference in the latency of simple and choice reactions be used to explain the results here reported. (Instructions to make precisely the same reaction to both lights and tones have very different psychological implications than do instructions to react to one and not to the other of two stimuli or to react differently to the two. Nor is there any evident connection between the present findings and the fact that a so-called 'motor set' has been found to give more rapid reactions than a so-called 'sensory set.')

The senior author (**9**) has elsewhere stressed the apparent delicacy of reaction time as a measure of preparatory set and of its conscious correlate, expectancy. A good illustration of this is to be observed in Fig. 2. In the part of the experimental procedure represented here, the first three changes from one type of stimulation to the other occurred after two series of tones and one series of lights which contained a not too highly irregular number of homogeneous presentations, ranging from seven to thirteen. However, the second series of lights contained 21 presentations. During this series the average reaction time dropped rapidly during the first six presentations, but then began to increase and continued to do so for the next eight presentations. This increase in reaction time is tentatively interpreted as meaning that after the first six presentations of the visual stimulus, the subjects began to expect, on the basis of their immediately preceding experience, a possible change to the auditory type of stimulus. This increased expectation of tone had the effect of lowering the degree of preparedness for reacting to light, with consequent lengthening of the latency of response to the latter type of stimulus. When, however, a change from lights to tones failed to occur by the 14th presentation of light, expectation of change underwent a gradual decay, as evidenced by the increasing speed of reaction to light from the 14th presentation on to the end of the series. The validity of this interpretation will be subjected to experimental test in the near future, at which time an attempt will also be made to determine whether the reaction-time measure of expectancy is more or less

sensitive than introspection. The reaction-time technique has, in any event, the very important advantage of lending itself to precise quantification, which introspection does not.

Since Prediction III is scarcely more than a correlary of Prediction II, which was conclusively verified by the results reported above, it is something of a mystery why Prediction III was not similarly confirmed. Inasmuch as this latter prediction was tested at the very end of the experimental procedure employed in this study, it may have been that fatigue, confusion, loss of interest, or some similar influence was operative. Or perhaps the succession of lights and tones, which were presented in simple alternation, was too short to establish a strong simple-alternation 'hypothesis.' A recheck of Prediction III is to be made shortly in which these possibilities will be examined.

Another perplexing feature of the results presented in Fig. 3 is that the average latency of response to the light presented on the 72nd trial was considerably shorter than might have been predicted. Prior to this point in the experiment, changes from one type of stimulus to the other had always been followed by a more or less protracted series of presentations of the same type of stimulus (see Fig. 1 and Fig. 2). It might have been supposed, therefore, that when, on the 71st trial, a tone was presented after a long, homogeneous series of lights, the subjects would have taken it for granted that there would now follow a long series of tones. Such an expectation, if it existed, should have materially increased the latency of response to a light presented at this time. It is true that the average latency of response to the light actually presented on the 72nd trial is somewhat longer than the average latencies of the several succeeding reactions to lights, but the latency of this response, occurring as it did to a presumably unexpected type of stimulus, might well have been as great or even greater than the latency of response to the tone that was presented on the 71st trial. It is interesting to note, incidentally, that the two tones presented in succession on the 89th and 90th trials were apparently sufficient to establish a fairly strong expectation of more tones, rather

than lights, as is indicated by the relatively long latency of response to the light presented on the 91st trial.

One of the most striking aspects of the results portrayed in Fig. 3 is the consistently longer latencies of the responses made to lights than of those made to tones. This finding has, however, no direct relevancy to the main objectives of the present study and merely confirms what has long been known concerning the relative latencies of visual and auditory responses.

Related Experiments

Of the various prior investigations that directly or indirectly support the main finding of the present study, namely, that preparatory set has important 'central' involvements which may vary independently of the position and muscular tonicity of the responding bodily member or members, perhaps the most relevant is that reported relatively recently by Hathaway (**5**). Using action-potential recording, this writer was unable to detect any changes in the muscles of the arm which coincided with the development of readiness for a simple reaction, following the presentation of a warning, or 'ready,' signal. However, as already pointed out, investigations of this kind, involving an attempt to show the presence or absence of muscular concomitants of preparatory set by direct examination of the responding member, are always open to the criticism that they were not sufficiently refined, that the technique employed was not definitive, and so forth. That a similar objection cannot be so readily made to the method of approach employed in the present investigation is an important advantage.[7]

Apparently the only prior use of the particular technique here described is that reported by Wundt (**11**) in 1880. The following quotation will show the basic similarity of findings as well as method.

> Slighter, but still very noticeable, is the retardation [in quickness of response] when the experiment is so arranged that the observer [subject], ignorant whether the stimulus is to be an impression of light, sound or touch, cannot keep his attention turned to any particular sense-organ in advance. One notices then at the same time a peculiar unrest like the feeling of strain which accompanies the attention keeps vacillating between the several senses (p. 243).

[7] An extended review of other similar studies has been presented elsewhere (**8**).

Although Wundt's observations in this connection were not systematically quantitative, it is clear from the foregoing passage that he took it for granted that the lengthening of reaction time noted under these conditions represented 'vacillations' in 'attention,' which he would presumably have identified as a 'central' phenomenon. Subsequent writers have sometimes attempted to make so-called 'selective attention,' *i.e.*, the differential reactivity of an organism to particular sensory stimuli, wholly dependent upon the motor adjustment of the various special sense organs. (Human beings as well as many other types of animals can, to a limited extent, select the stimuli to which they respond by means of sense-organ manipulation.) These adjustments are largely muscular in character and thus provide a possible basis for a 'motor' theory of preparatory set (attention, expectancy) which stresses the receptor rather than the effector end of the response arch as the locus of this phenomenon. However, as already demonstrated by the findings of the present investigation, 'selective attention' may be prominently manifested by human subjects under experimental conditions in which no concomitant changes in sense-organ adjustment are evident. If this phenomenon cannot be correlated with the motor adjustments of either sensory or reaction mechanisms, as the present findings suggest, the only remaining alternative is the conclusion that it has a 'central' locus.

Summary

On the basis of the hypothesis that preparatory set (expectancy, attention) is mediated principally by neural rather than by neuro-muscular mechanisms, three predictions are made. The results of an experiment designed to test these predictions unambiguously confirm Prediction I and Prediction II, but they do not confirm Prediction III. Conjectured reasons for the non-confirmation of this latter prediction are offered and proposals made for their experimental verification.

The delicacy of reaction time as a measure of preparatory set is stressed and exemplified. Other incidental features of

the findings of the present investigation are noted and briefly discussed.

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THE ROLE OF EYE MOVEMENTS IN THE AUTOKINETIC PHENOMENON *

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When a stationary point of light is fixated in the dark, there occurs for nearly all observers an illusion of movement, which is commonly known as the autokinetic phenomenon. Although the illusion was first reported in 1799, there is no accepted explanation. The obvious and simplest explanation in terms of involuntary eye movements was proposed as early as 1879 by Hoppe (**9**), and was endorsed by many subsequent writers. At the present time, however, investigators are agreed in rejecting the eye-movement hypothesis on the basis of experimental results and of logical arguments.[1] It is the purpose of this paper to reconsider the eye-movement hypothesis as an explanation of the autokinetic phenomenon.

The possibility that *large* involuntary eye movements influenced the illusion was tested by Charpentier (**5**) in 1886. He arranged dots of light in a group so that only the central dots were visible when the observer fixated. If any large eye movements occurred, dots beyond the periphery of the original visual field would come into view. Charpentier found, however, that the central group appeared to move without any change in pattern, and concluded that large involuntary eye movements did not account for the illusion. The same conclusion was reached by Bourdon (**2**) whose technique was to turn on the general illumination after the light-spot had appeared to move to some considerable distance. He found that the eye still fixated the stimulus-object. Marx (**11**), also, had evidence against large eye

* From The Psychological Laboratory of Princeton University.

[1] The history of the experimental studies of the autokinetic phenomenon has been treated fully by Adams (**1**). The development of the eye-movement hypothesis has been summarized by Guilford and Dallenbach (**8**).

movements as the cause of the illusion. While the subject was observing the illusory movement, Marx suddenly fully opened a shutter around the fixated point of light, thus exposing a glowing filament which produced an after-image. The subject then fixated a dot on a piece of paper, and marked on the paper the point of projection of the after-image. If the eye moved during the illusion, the projected after-image would be displaced from the dot. But it was found that in 80 observations, fixation was quite exact 30 times, and in the other cases the deviations were small. Further evidence on the same point is that of Ziehen (**14**) who found in the cases of two hemianopics that the stimulus-object, at times, appeared to have moved into the blind half of the visual field.

In opposition to these conclusions, Carr (**3**) found that large eye movements could produce illusory movement. However, Carr believed that the apparent movement produced by large movements differed from the autokinetic phenomenon as it is usually defined. In the former case, the spot of light moved away from the apparent fixation point. In the latter case, spot and apparent fixation point moved together.

That large involuntary eye movements are not necessary to produce the autokinetic effect seems well established. Nevertheless, the possibility remains that when such movements do occur they may have a marked effect on the illusion.

That *small* involuntary eye movements are effective in producing the autokinetic phenomenon is also possible. Guilford and Dallenbach (**7**), however, believed that Hunter has provided logical proof that apparent movement cannot be due to nystagmus of small amplitude. Hunter (**10**) was primarily concerned with the after-effect of visually observed motion, and gives the following reasons why retinal displacements due to nystagmus could not condition the after-effect:

1. Where the nystagmic sweep of the eye is wide, one should be able to duplicate the retinal displacements voluntarily. Such voluntarily initiated eye-movements do not result in the customary after-movement. This is taking for granted that a retinal displacement produced by a voluntary act has the same meaning as one produced by a reflex act.

2. If large movements cannot produce the required phenomenon, it seems unwarranted to assume that nystagmic movements of microscopic extent would do so by the retinal displacements involved.

Ziehen (**14**), specifically considering the autokinetic phenomenon, attempted to see if very small voluntary eye movements in a definite direction called forth illusory movement in this direction. The result was negative.

The argument that would eliminate the eye-movement hypothesis on the ground that the illusion cannot be reproduced by voluntary nystagmic eye movements is striking in its weakness. The eye-movement hypothesis, whenever specifically elaborated, is concerned with involuntary eye movements. Logically, the first test of the hypothesis is to determine if the illusion occurs when involuntary eye movements are induced. Such a test is easily made by employing any technique which induces nystagmus, as is shown in the following experiment.

Rotational and Caloric Nystagmus

Experiment 1

A point of light, obtained by masking incompletely the bulb of a small flashlight, was attached to a motor-driven rotating chair. The light was set up in the line of regard of the subject, and at a distance of one meter from his eye.

While fixating the light attached to the chair the subject, in each trial, was rotated 15 times. Rotation was in the counterclockwise direction, so as to produce an after-nystagmus with the first phase to the right. When rotation had ceased, the subject, still fixating the light, was required to report on his visual experiences.

Nine subjects were used in a total of 16 trials. In every case, it was reported that after the cessation of rotation, the spot moved to the right (and continued to do so for some seconds). The reports varied only in that sometimes the movement was smooth, and at other times jerky; for one subject the spot jumped back and forth, but with a greater tendency to the right than to the left.

The results of this experiment are consistent with the

visual experiences that ordinarily occur after rotation. However, the subjective experiences during and after rotation are complicated by the fact that the visual experience of movement is often accompanied by the sensation of bodily rotation.

This difficulty can be avoided by eliciting nystagmus through caloric rather than rotational stimulation. Unilateral irrigation of the ear with cold water (14 to 20 degrees Centigrade) evoked illusory movement of the spot of light in the contralateral direction. This was consistently found to be true with 10 subjects who were submitted to a total of 49 trials. The effect is striking. If care is taken to stop the irrigation a few seconds after the subject reports the start of the illusion, over-stimulation, with its concomitant bodily disturbance, is prevented. The naive subject has no idea that his eyes have been set into activity; he is aware only of the rapid movement of the spot of light. Phenomenally, the experience does not differ from the autokinetic effect.

These simple investigations with rotational and caloric nystagmus show clearly that the autokinetic phenomenon *can* be due to involuntary eye movement.

On Exner's Observation

Nevertheless, a significant objection to any eye-movement hypothesis is raised by an experiment of Exner (**7**). His apparatus was simply a small black disc, mounted on white cardboard, with a pinhole in the center. The light through the pinhole and the illumination of the room were regulated so that the black spot, with the point of light in its center, could just be seen clearly. Exner found that when the point of light was fixated, an illusion could occur in which the light moved about within the disc, never reaching the boundary, while the disc moved in its own way. Independent movement of the spot and disc cannot be due to eye movement, since eye movement would displace the pattern as a whole upon the retina.

Exner's observation has been verified by Schilder (**12**), but other investigatiors have failed to confirm it. Bourdon (**2**), using a luminous circle enclosing a bright cross made up of

9 points of light, found that the figure moved as a whole. However, this arrangement differs considerably from that used by Exner. Simon (**13**) also failed to confirm the observation. He observed, in a dark room, a bright point of light in a dimly luminous disc, and reported that the light and disc moved together. Ziehen replaced Exner's disc with a square in order to make observation easier, and reported that small movements of the spot within the square occurred rather often, but that usually the spot and square moved together.

Thus, Exner's observation is not firmly established; but if the spot and disc can behave independently in autokinetic movement, the fact would appear to be a crucial argument against the possibility that eye movements condition the illusion. Therefore, the following experiment was undertaken:

Experiment 2

A preliminary exploration made it apparent that Exner's observation could be confirmed by fixating, in a dark room, a bright spot within a dimly luminous disc. This technique, originally used by Simon, yielded him negative results.

There was some evidence in this preliminary work that the independent movement of the spot and the disc was not of the same nature as the autokinetic phenomenon. Parts of the disc seemed to fade out and reappear. Thus, it is possible that the displacement of the spot relative to the disc might be explained in terms of the process of light adaptation. It is suggested that if the eye adapts to the entire luminous pattern of spot and disc, then, with the slight shift of the eye to refixate the spot, there would be a shift of the retinal area that is stimulated by the disc. Thus, part of the disc would stimulate a new region of the retina and be quite visible; the rest of the disc would stimulate an adapted region of the retina, so that it would not be visible or would be indistinct. With parts of the disc fading out and reappearing, the spot would lose its central relation to the disc, and would give illusory movement.

It seemed possible that a partial test of the adaptation

hypothesis could be made by replacing the customary disc with a luminous circle. The rim of the circle offers much less area as a stimulus than does the surface of the disc. It follows from the adaptation hypothesis that the illusion should be absent or at least reduced under these circumstances.

Therefore, the Exner experiment was modified by using as stimuli: (1) a bright point of light in the center of a dimly luminous *disc* of 1.3 cm diameter and (2) a bright point of light in the center of a luminous *circle* of 1.3 cm inner diameter, and 1.5 cm outer diameter. Ten subjects, all of whom had had some experience in observing the autokinetic phenomenon, made observations on each stimulus and reported any movement perceived in the plane of the pattern.

Every subject reported independent movement of the spot and disc, although in no case did the spot leave the disc. Some subjects spoke of the spot as moving within the disc; others maintained that the spot was stationary, while the disc moved about it. Most subjects also saw readily the pattern of spot and disc moving as a whole. For the spot and circle, on the other hand, ever subject reported that the pattern moved as a whole. Two subjects were occasionally able to see, in addition, very slight oscillation of the spot within the circle.

Exner's observation of independent movement of spot and disc is, then, amply confirmed. But in the observations of the spot within the circle, this effect was so much reduced that only two of the ten subjects reported it as occurring even slightly. This result favors the adaptation hypothesis. Further evidence comes from the introspective reports of the observers. Nine subjects reported that the disc changed in shape; of these, 3 specifically stated that the disc faded on one side.

The result of this experiment indicates that Exner's original observation does not necessarily invalidate the eye-movement hypothesis for the autokinetic phenomenon.

Photographic Studies of the Autokinetic Phenomenon

In addition to the indirect tests reported above, a direct photographic study of the role of eye movements in the autokinetic phenomenon was carried out by Guilford and Dallenbach. The corneal reflections from the eyes of the subjects were photographed while the illusion was occurring. During the course and at the close of the experiments the subject made drawings of the path of the illusory movement of the spot of light, for comparison with the photographic records. The writers reported that the photographic records showed no large movements of the eye during the illusion. They found the small eye movements to be too limited in extent to account for the large illusory movements of the light, and too irregular and too few in number to account for the small oscillatory movements of the light. They concluded, therefore, that eye movements are not essential conditions of the phenomenon.

The significance of the negative results of this photographic study is, however, limited. In the first place, only the horizontal component of the eye movements was recorded by the photographic technique, whereas the direction ordinarily taken by the illusion is very largely vertical. This difficulty is true for many of the records published by Guilford and Dallenbach. In the second place, it should be pointed out that the sensitivity of their recording instrument was 0.7 mm displacement of the eye line for one degree of voluntary eye movement. This is a sensitive instrument, but its sensitivity in the detection of eye movements compares unfavorably with the sensitivity of the eye in detecting the movement of a point of light. The threshold for the perception of real movement of a point of light in a homogeneous field, as cited by Duke-Elder (**6**), is about 10 minutes of arc per second. If the eye must move a comparable amount per second to produce an illusion of movement, then a single eye movement producing a minimal illusion would be indicated on the record as a displacement of .12 mm per second. Furthermore a true test of the eye-movement hypothesis must take into account the possibility that the illusion results from the summation of the effects of a rapid series of nystagmic eye movements.

It would seem, then, that the technique of Guilford and Dallenbach should be applied under conditions in which the direction of the illusion is controlled (for a maximum horizontal component) and in which the speed of the illusion is marked, before it can legitimately be concluded that eye movements are not essential to the autokinetic phenomenon.

Experiment 3

The next phase of this research deals with the photographic study of eye and head movements during the perception of the autokinetic phenomenon under different conditions.

The photographic records were obtained with the ophthalmograph (American Optical Company), ordinarily used for the study of eye movements in reading. This instrument simultaneously photographs corneal reflections from both eyes, producing two eye lines on continuous film. A head record was substituted for one of the eye lines by the following procedure: The subject was required to wear rubber goggles from which the glasses had been removed. A pasteboard blind, upon which was mounted a little glass sphere, was set into one of the windows. The glass bead concentrated and reflected the beam of light from the ophthalmograph, producing a fine line indicative of head movement.

The camera was modified to give both a time record and also a signal record. The camera lights were masked so that they did not illuminate the apparatus and room. Since the light shone into the eyes from below, there was considerable glare in the lower portion of the visual field. The rest of the field, however, was homogeneously dark, and when a point of light was introduced into it, the autokinetic effect readily appeared.

The rated sensitivity of the ophthalmograph is 1 mm displacement of the eye line per 2 degrees of eye movement. The instrument, then, is somewhat less sensitive than that of Guilford and Dallenbach.

Part 1.—Twenty-five records were obtained from 21 subjects while they observed the ordinary autokinetic phenomenon in the horizontal direction. Nine of these records

revealed appropriate eye movements, showing nystagmus with the fast phase in the same direction as the illusion. Fourteen records were of questionable significance, showing considerable eye movement, but no clear nystagmus that

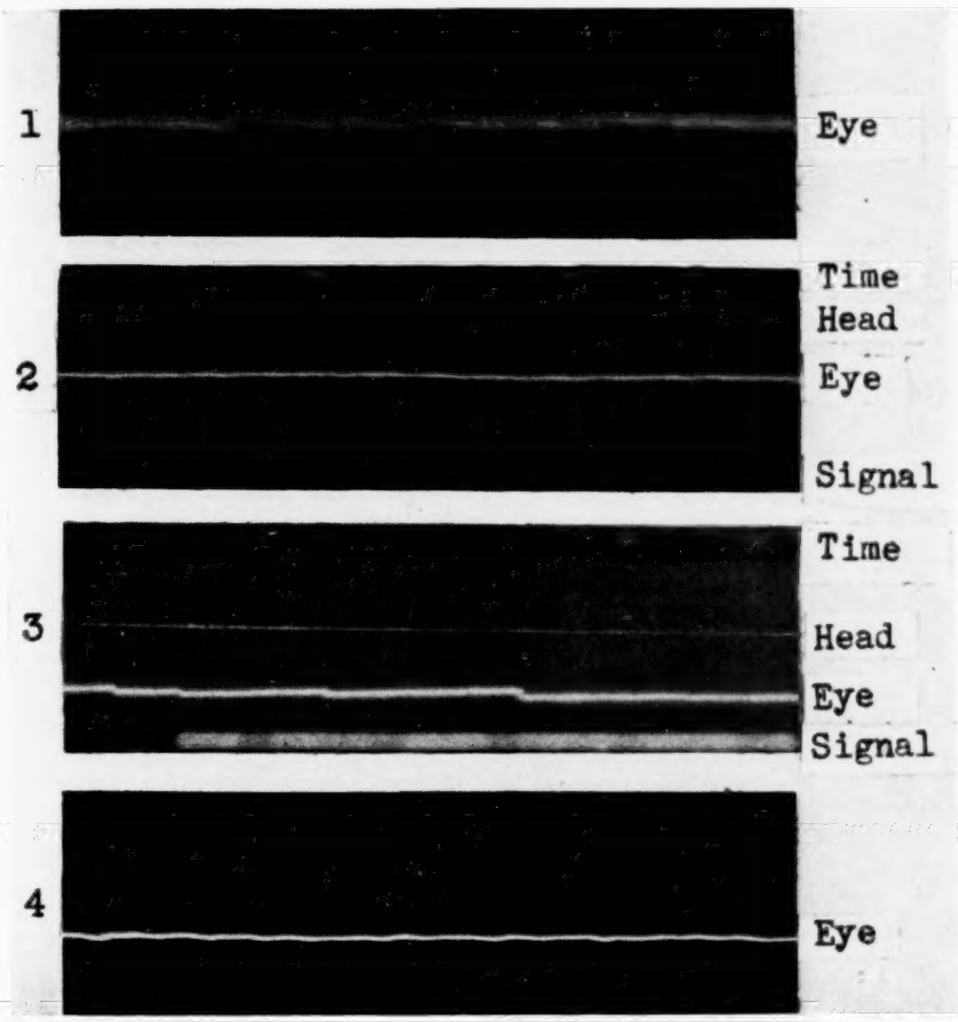

FIG. 1. An eye-movement record taken during observation of the autokinetic phenomenon. The frequency of the nystagmus is 6 cycles per second, and its fast phase is in the same direction as the apparent movement of the light-spot.

All the records read from left to right. An upward deflection of the eye-line indicates an eye-movement to the right; downward to the left. The records are all in the original size except for this figure which is magnified 5 times.

FIG. 2. A sample of an eye record taken in the period of recovery from strain of 30 degrees. The fast phase of the nystagmus is in the same direction as the illusion reported by the subject. The time line is in seconds.

FIG. 3. Portion of a record taken in the period of recovery from an eye strain of 50 degrees. The fast phase of the nystagmus is in the direction of the illusion. The head lines are retouched.

FIG. 4. Portion of a record taken in the period of recovery from a strain of 50 degrees. The record is questionable, since the relation of the activity of the eye to the direction of the illusion is not apparent.

would determine the direction of the illusion. The remaining 2 records showed very little eye movement. Figure 1 is an example of appropriate eye movement. Careful examination reveals a tiny nystagmus; the fast phase is to the right, corresponding to the direction of the illusion. The deflections are slight and not very apparent to direct examinations, but they are convincingly revealed under the microscope.

Part 2.—When the eye is voluntarily turned to one side and is held there, a marked autokinetic illusion results which is in the same direction as the eye strain. On cessation of the strain, an illusion usually occurs in the direction opposite to the original strain provided the eye was turned through a large angle (**3**). The purpose of this experiment was to determine if the condition of relaxation after eye strain, which produces a striking autokinetic effect, would give corresponding photographic records.

Spots of light, individually controlled, were so placed that the lines from them to the center of the subject's eye formed angles of 25, 30, 50 or 60 degrees with the original direct line of regard. When a particular light was turned on, the subject was required to maintain an eye strain at the determined angle for 30 seconds. Then this spot was turned off, and the spot directly in front of the subject, which the subject was directed to fixate, was turned on. Photographic records were taken while the subject observed the illusion in this period of recovery from strain.

A record was made on each of 10 subjects during the period following a 25 or 30 degree strain.[2] Seven subjects reported the illusion. Three of their records showed appropriate eye movements; 4 were questionable. The remaining 3 subjects reported no illusion and the records showed very little eye movement. Figure 2 is an example of a positive record.

Thirty-four records on 26 subjects were made of the period

[2] The 25 degree angle was introduced into the experiment as an alternative to 30 degrees when it was found that the headrest of the camera obstructed the vision of some subjects for the latter angle. Strain at 60 degrees seemed too severe to some subjects, so it was discontinued after a few trials. The records at this angle were lumped with those of 50 degrees, since no difference in the results was apparent.

following a 50 or 60 degree strain. The illusion was reported in 28 cases. Twenty-four of the corresponding records showed appropriate eye movements and 4 were questionable. In the 6 instances in which no illusion was observed, very little eye movement was indicated by the record. Figure 3 illustrates the results obtained when the illusion was marked. Figure 4 is an example of a questionable record.

The results of these experiments indicate that under conditions that tend to make the autokinetic effect more marked, appropriate eye movements are detected more readily. Positive records are more frequent and the nystagmus shown is greater in extent. The series of Figures 1, 2, and 3, illustrates the increased amplitude of nystagmus that accompanies the effects of increased strain. It has been maintained (**4**) that the eye does not move in the period of recovery from strain. The experimental results refute this contention, although it is not certain that eye movements are always to be found in the recovery period.

After-image Superimposed on the Fixated Light

A significant objection to the eye-movement hypothesis arises from the observation of Carr (**3**) that an after-image projected onto a point of light participates in the illusory movement. Any movement of the eye would separate the spot from the after-image, since the spot would be displaced on the retina while the after-image maintained a fixed retinal position. The following experiment is a study of simultaneous movement of spot and after-image.

Experiment 4

Part 1.—It has been demonstrated that illusory movement of the spot, indistinguishable from the ordinary autokinetic illusion, accompanies nystagmus. The purpose of this part of the experiment was to determine the behavior of a spot and superimposed after-image during nystagmus.

The rotating chair of Experiment 1 was used. An after-image of an upright cross within a circle was formed by exposing the eyes of the subject to an illuminated electric light

bulb masked to produce this pattern. In each trial, when the after-image had been formed, the subject was rotated 15 times clockwise. The subject was required to report on all of his experience throughout the experiment, but particularly on the movement of the spot and after-image after cessation of rotation.

Nine subjects were used. The distance of the light bulb from the eye of the subject was usually one meter. The distance was changed to 2 meters in a number of trials, to see if changing the size of the after-image would markedly influence the results.

At either distance of the after-image, it was found that any of 3 effects could occur:

1. The spot moved to the left, and simultaneously the after-image drifted to the right. (18 reports.)
2. The after-image remained stationary, and the spot jumped jerkily to the left within it. (11 reports.)
3. The spot and the after-image moved together to the left. (9 reports.).

Effects 1 and 2 lend themselves readily to explanation. In effect 1, in which the spot and after-image separate, nystagmus would explain the illusory movement of the spot. A large drift of the eyes might account for the drift of the after-image. The drift would also increase the illusory movement of the spot. The subjects were aware of the large eye movement when it occurred, and could inhibit the drift of the after-image by voluntary effort. In effect 2, in which the after-image remains stationary, the movement of the spot can be explained in terms of simple nystagmus, which would leave the after-image unaffected. In effect 3, contrary to the expectation that eye movement must separate spot and after-image, is revealed the interesting fact that simultaneous movement of the two can occur under conditions that are calculated to produce involuntary eye movements. This phenomenon is treated objectively in part 2 of this experiment.

Part 2.—The purpose of this part of the experiment was to study the horizontal eye and head movements of the subject

while he observed horizontal movement of spot and superimposed after-image.

The effect was studied in the period of recovery from eye strain rather than in the period after rotation, in order to simplify the photographic technique. An after-image was formed on the right eye of the subject, who was then required to strain his eyes at an angle of 50 degrees for 30 seconds. Next, the subject fixated a spot directly in front of him (zero degrees), and photographic records were taken while he observed the illusory movement during this recovery period.

Eleven records were obtained, with 3 subjects, while they observed horizontal movement of the spot and superimposed after-image. In every case, nystagmus was evident in the eye line. Furthermore, as can be seen in Figure 5, large drifts of both the head and the eye lines occurred.

Special tests showed that the large drift of the eye line, when it accompanied a similar drift of the head line, did not necessarily indicate a loss of fixation. Movement of the head alters the geometrical relations between the eye, camera, and beam of light in such a manner that the eye record is shifted in the same direction in which the head is displaced, even though the eye maintains its fixation.

Because of the important role played by head movements in this group of records, additional tests were made with one subject in which a biting board was used to keep the head steady. In preliminary trials during the period of recovery after strain to the right, it was found that the after-image remained stationary, while the spot jumped jerkily to the left within the after-image. (*Cf.* effect 2 of Experiment 2.) After a few trials, however, the subject was able to obtain a simultaneous movement of spot and after-image even under these conditions. Three records were then taken. In each case, the eye line showed a great deal of activity, as shown in Figure 6. The head line was steady in two of the records but showed a little drift in the third.

The trials in which the head was not restrained by the biting board yielded results which are easily interpreted. As in the cases above, the nystagmic eye movements can account

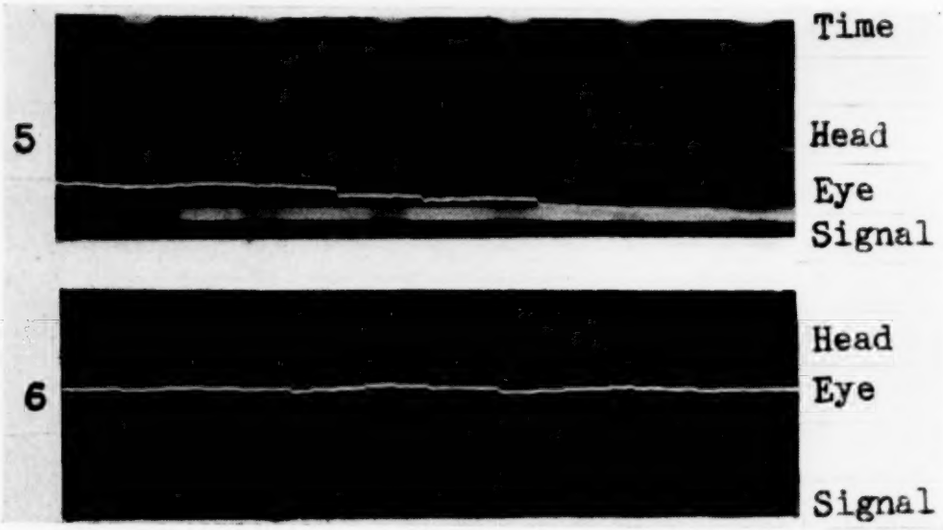

FIG. 5. A record, showing head and eye lines, taken in the period of recovery from a strain of 50 degrees. The subject was observing simultaneous movement of a spot and superimposed after-image. The head of the subject was unrestrained.

FIG. 6. A record taken in the period of recovery from strain, during observation of the simultaneous movement of a spot and superimposed after-image. A biting board was used to restrain the head of the subject. Head lines are retouched.

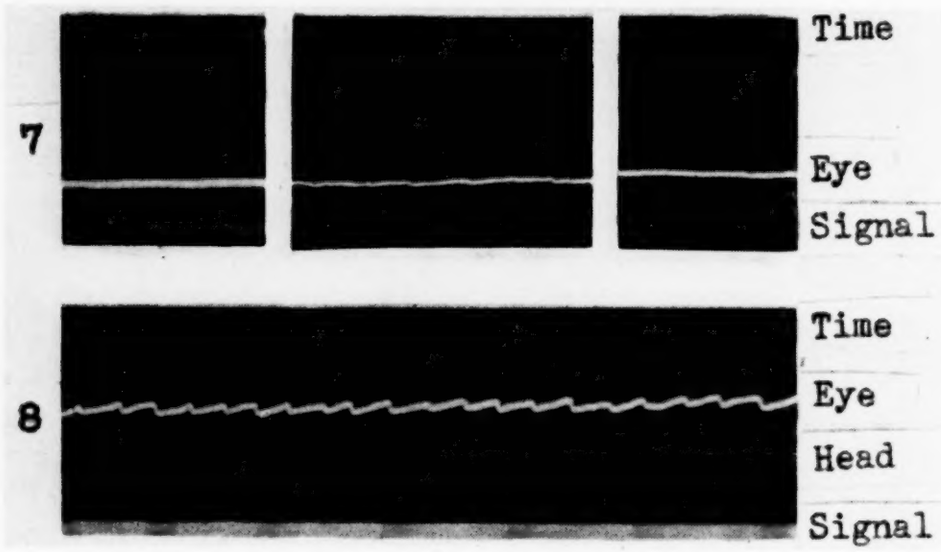

FIG. 7. Portions from a typical record of eye movement during caloric irrigation. The section on the left, from the beginning of the record, shows no nystagmus although the illusory movement of the spot had started. The section in the center, from the middle of the record, shows a slow nystagmus. The section on the right, from the end of the record, shows no nystagmus; but the illusion, although reduced, was still present.

FIG. 8. Portion of a record of caloric irrigation which shows a very marked nystagmus.

for the movement of the spot. Then, it is known that as the head is rotated, an "after-image will always be seen in front of the head, and consequently its position in relation to the trunk of the body will vary as the head is moved" (**4**). Thus, as the head rotates in the direction of the illusory movement of the spot, the after-image moves in the same direction. So long as the spot is fixated (within the limits of the small nystagmus), only minor displacements of the foveal after-image and spot, relative to one another, can occur.

The activity of the eye that occurred when the head was restrained took no definite form that would lead to a ready interpretation. However, nystagmic eye movements again might account for the movement of the spot. But to account for the drift of the after-image, it would seem that the eye must assume the function that otherwise is performed by the unrestrained head. It may be that drift of the eye in pursuit of the illusion accounts for the movement of the after-image. If the drifts are short, and are followed by rapid involuntary refixations, the displacements of the spot within the after-image would be very slight. Such oscillatory displacements frequently are reported, but they are not very pronounced in the complex perceptual situation that involves the observation of marked simultaneous movement of spot and after-image. Figure 6 is a sample record which might well be consistent with the interpretation of the eye line as a summation of semi-voluntary drifts and involuntary eye movements.

As has been pointed out earlier, the argument has been advanced that the simultaneous movement of spot and after-image eliminates the possibility that eye movement occurs during the autokinetic phenomenon. This argument is refuted by the work on rotation and by direct photographic evidence, thus restoring as a possibility the eye-movement hypothesis.

Caloric Nystagmus

It will be remembered that what appears to be the autokinetic illusion can be elicited by caloric irrigation which is known to produce nystagmus. The illusory movement of the

spot is in the same direction as the fast phase of the nystagmus to be expected from the irrigation. Since the illusory movement of the spot starts up slowly, gathers speed, and finally subsides, it is important to determine the temporal relations between the nystagmus and the illusion.

Experiment 5

Ten photographic records were obtained on three subjects during observation of the illusion induced by irrigation of the ear with cold water. In order that the records of caloric nystagmus might vary, a range of temperatures from 15.5 to 20 degrees Centigrade was used. To insure a marked effect in each trial, the caloric stimulation was continued for 20 seconds after the subject had signalled the beginning of the illusion.

The typical record, illustrated in Figure 7, shows no observabable nystagmus during the early part of the illusion. Some time after the spot has begun to move, nystagmus appears and rises in amplitude. Then the nystagmus slowly diminishes. The fast phase of the nystagmus is in the direction of the illusory movement. During the latter part of the illusion, nystagmus is no longer observable in the record.

The records, however, vary. In one case of very marked illusion, Figure 8, nystagmus was found throughout the record, following the pattern of a rise to a maximum and then gradual subsidence. At the other extreme, one record shows no nystagmus at all, although the illusion was present.

It is clear that nystagmus and illusory movement are closely related. Although nystagmus is not always observable, the direction of the illusion is always the same as the fast phase of the nystagmus when it occurs, and is consistent with the direction of expected nystagmus when it is not detected. The results of this experiment, furthermore, are consistent with those presented above: positive records of eye movement are easily obtained during marked phases of illusory movement. When the illusion is not marked the records of eye movements frequently become questionable or negative.

It is more than possible that the photographic technique is not adequate to a full solution of the problem of the role of eye movements in the autokinetic phenomenon. The sensitivity of the method, as was pointed out above, is not great as compared with the sensitivity of the eye.[3] It is exceedingly difficult, moreover, to integrate the tiny deviations of the eye line on a record which is frequently marred by the intrusion of presumably extraneous, random movements of the eye and complicated by movements of the head.

Direct Observation

Some of these difficulties can be overcome by direct observation of the corneal reflection on the ground glass focussing screen of the ophthalmograph. The eye of the observer can integrate the repeated excursions of the nystagmus and can ignore some of the extraneous fluctuations. Furthermore, the method of direct observation has the advantage that the eye movements can appropriately be studied during illusions of any direction. A similar approach was used by Carr (**3**), who observed the eye directly, sighting through a system of threads. He concluded that in the ordinary illusion no movements of the eye-ball, other than irregular spasmodic twitchings, occur. But the present observations led to the following experiment:

Experiment 6

The subject was seated at the ophthalmograph in the usual manner. The experimenter observed the corneal reflection on the focussing screen, and when the movement of the corneal reflection indicated nystagmus or large involuntary drifts of the eye, he called out the probable direction of the illusion. The subject then responded with the actual direction. Records were kept by an assistant. The discrimination of direction was limited to 8 possibilities; up, up-right, right, right-down, down, down-left, left, and left-up. The experimenter was also permitted the category 'station-

[3] An attempt was made to study eye movements during the illusion by amplifying the electrical changes that occur during rotation of the eyeball. The electrical method was not found to be as satisfactory as the photographic method for the detection of very small eye movements.

ary' for pauses in the illusion. Ten calls constituted a series. The call of the experimenter can agree in part with the report of the subject. It might be reasonable to score such calls as half-correct since the judgment is determined by the consideration of two factors, the phase of nystagmus and the direction of large drift. The data actually were scored as correct, half-correct, and incorrect. But to avoid undue complication in comparing the results with the scores that could be expected by chance, partially correct calls have been treated as incorrect. A precise determination of what constitutes a chance score is not possible due to the freedom in time permitted to the experimenter in calling his observations. But as the calls fall into 9 categories, we may consider that a chance score would be about 11 percent. Series of calls were made during observation of the ordinary autokinetic illusion and also under conditions of recovery from eye strain. Four unselected undergraduates (majors in Psychology) and the writer acted as experimenters. Twenty-six subjects were used. The gross data totalled 790 calls: 479 correct, 116 half-correct, 195 incorrect. Thus, of the total, 60.6 percent were fully correct.

Due to the fact that the illusion persists in any direction for an unpredictable but appreciable length of time, it was possible that the experimenter, by repeated calling, could make runs of accurate judgments. To eliminate this possibility, every call which repeated, either wholly or in part, the previous report of the subject was discounted. The calls then totalled 341: 195 correct, 36 half-correct, 110 incorrect. Thus, 57.2 percent were fully correct.

The scores are considerably higher than chance though they fall far short of perfection. It must be remembered, however, that the judgments are difficult; not only must extraneous fluctuations of the corneal spot be discounted, but the discrimination between the fast and slow phases of a rapid and small nystagmus can easily be erroneous. There seemed, also, to be individual differences in the ability of the experimenters to make the observations. One experimenter was consistently better than the others. Another experi-

menter became irritated with the task, and his scores were low.

On the other hand, there is the possibility that the report of the subject was influenced by that of the experimenter. Therefore, a variation of the procedure was substituted during 22 series of the ordinary autokinetic illusion. When the subject was satisfied that the illusion was pursuing a definite direction, he flashed a signal demanding that the experimenter make a report. The experimenter then recorded his observation in writing. Finally, the subject announced the direction of the illusion. These tests totalled 220 calls; 85 correct, 61 half-correct, 74 incorrect. The number correct is 38.6 percent of the total. This procedure, then, shows a marked drop in the accuracy of calling. But the loss can hardly be attributed entirely to the factor of suggestion. The undergraduate experimenters objected to the introduction of this difficult procedure which required constant fixation of the spot on the focussing screen, and the ability to make a very rapid judgment on the basis of the movements of the corneal spot that had occurred prior to those occupying the attention of the experimenter. Nevertheless, the best experimenter was not greatly disturbed by this supplementary procedure. His calls totalled 60: 29 correct, 25 half-correct, 6 incorrect. Thus, 48.3 percent were fully correct.

The results of this experiment are brought together in Table 1. This table shows that the scores for the illusion in

TABLE 1

Recovery from Angle of	Gross Data				Repeats Eliminated			
	All Experimenters		Best Experimenter		All Experimenters		Best Experimenter	
	No. of Calls	% fully Correct	No. of Calls	% fully Correct	No. of Calls	% fully Correct	No. of Calls	% fully Correct
50–60 deg.....	331	61.8	111	70.2	155	61.9	37	62.2
25–30 deg.....	281	58.7	81	71.6	123	53.4	32	62.5
0 deg.....	178	61.2	30	60.0	63	54.0	10	60.0
Total.....	790		222		341		79	
Average.....		60.6		68.4		57.2		62.0
Control for Suggestion....	220	38.6	60	48.3				

the period of recovery from strain do not differ markedly from the scores at zero degrees. This is due to the fact that the definite and predictable direction of the recovery period lasts but a few seconds, and then the illusion assumes the random movement of the ordinary autokinetic phenomenon. When this is taken into consideration, the results of the method of direct observation of eye movements compare very favorably with the photographic results.

It is concluded that the 'irregular spasmodic twitching' that previously has been found in the direct observation of the eye during the autokinetic phenomenon is significantly regular.

Summary

In this experimental study of the autokinetic phenomenon, the eye-movement hypothesis has been re-examined. The following results have been obtained:

1. Illusory movement of a spot of light which cannot be distinguished by the subject from the ordinary autokinetic phenomenon is produced by stimulation which is known to induce nystagmus.

2. Exner's observation of independent movement of a disc and pinhole at its center is confirmed. The results of our experiments indicate that this phenomenon is not necessarily an argument against the eye-movement hypothesis. It seems, rather, that Exner's results can be explained in terms of the process of light adaptation.

3. A photographic study of the autokinetic phenomenon demonstrated appropriate eye movements in a number of cases. Under conditions which produced a marked autokinetic effect, the eye movements are striking.

4. It was demonstrated that apparent movement of a spot and of a superimposed after-image can occur together when the eyes of the subject are moving. Such simultaneous movement can occur in the period after rotational stimulation. The photographic technique reveals eye and head movements during observation of the effect in the period of recovery from strain. When movement of the head is restrained, the eyes are very active.

5. The insensitivity of the photographic method in detecting eye movement relative to the sensitivity of the eye in detecting movement of a spot of light, is indicated by theoretical considerations, and by the frequent failure of the photographic technique to reveal nystagmus when illusory movement was induced by caloric irrigation.

6. Direct observation of the corneal reflection in the focussing screen of the ophthalmograph, makes the nystagmic twitching of the eye directly apparent. The experimenter is able to determine the direction of the autokinetic illusion with an accuracy that well exceeds chance expectation. In the study of the ordinary autokinetic effect, the method of direct observation proves to be more sensitive than the photographic technique.

The experimental reconsideration of the eye-movement hypothesis for the autokinetic phenomenon shows that the arguments levelled against the hypothesis are invalid. On the other hand, considerable evidence in favor of the hypothesis is revealed.

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A COMPARISON OF HUMAN AND CHIMPANZEE SPECTRAL HUE DISCRIMINATION CURVES [1]

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I. Introduction

It has been well established by the studies of König and Dieterici (1892), Steindler (1906), Laurens and Hamilton (1923), and Wright and Pitt (1934) that the typical curve of human hue discrimination as a function of wave-length is characterized by primary minima in the yellow and blue-green regions of the spectrum. For the occurrence of these minima there is a satisfactory explanation in the three sensation curves of the Young-Helmholtz theory. The most rapid hue changes in the spectrum occur where the stimulation of one type of receptor is falling off rapidly and that of another type is increasing, roughly at the crossing points of the blue and green, and green and red sensation curves. All of the above investigators found a secondary minimum in the violet region. This can be explained by the secondary rise of the red sensation curve at the short wave-length end of the spectrum. Steindler, and Laurens and Hamilton found another secondary minimum in the red region. Since no such secondary dip was found by König and Dieterici, and Wright and Pitt, its validity is somewhat doubtful.

In contrast to the hue discrimination curves for normal trichromatic vision, Hecht and Shlaer (1936) have shown the curves of dichromats (two deuteranopes and one protanope) to contain only one region of fine discrimination, roughly equivalent to the blue-green minimum for trichromats. Steindler's (1906) finding of a protanopic minimum in the

[1] The author wishes to express his gratitude to Doctor Robert M. Yerkes for making possible the construction of the spectral apparatus needed in this study and for offering suggestions which were helpful in the execution of the experiment. The author is also indebted to Doctor Henry W. Nissen for much valuable advice.

yellow region is negated by lack of brightness control. The single minimum for dichromats corresponds in wave-length to the crossing point of the two sensation curves assumed by the Young-Helmholtz theory to explain this type of color blindness. It would seem, therefore, that the spectral hue discrimination curve can be of considerable aid in an analysis of visual receptor mechanisms.

Hitherto the only use of the spectral hue discrimination curve for the analysis of vision in an animal has been by Hamilton and Coleman (1933) for the pigeon. They found this bird to have maximum hue discrimination in the same two regions as do human beings. On the basis of this finding these investigators concluded that the visual mechanism of the pigeon probably was trichromatic.

The purpose of the present experiment was to compare the spectral hue discrimination curves of chimpanzees and human beings, and thus to further our understanding of the evolutionary history of human color vision. Earlier work toward this end by the author (1939) has uncovered evidence of dichromatic vision in cebus monkeys. The author also found evidence that in rhesus monkeys (1939) and chimpanzees (in press, *a*) vision for the long wave-length end of the spectrum is slightly less well developed than in man, but that in chimpanzees (in press, *b*), at least, there is no shortening of the spectrum.

II. Apparatus, Technique, and Subjects

The specific apparatus used in this study has been described in another article (in press, *a*), and more detailed features of the monochromators used to provide spectral colors have been given in two earlier publications (Grether, 1935; 1939). Briefly, spectral colors projected upon two separate diffusing surfaces were viewed through two glass windows. By pushing against these windows the animals gained access to food rewards.

The present experiment followed one (in press, *a*) in which difference limens had had been measured at three spectral points, 640 mu, 589 mu, and 500 mu. In the preceding study limens had been determined to within one millimicron of wave-length difference without interpolation. In the present experiment each limen was based upon a single series of five diminishing wave-length differences, with a total of 25 trials at each difference. Experimental sessions consisted of 25 trials, five trials on each color pair of the series. The point at which a discrimination curve based upon such a series crossed the criterion line (*i.e.*, linear interpolation) determined each limen. Except for the limen farthest in the red, the two points (one above and one below the

criterion line) used for linear interpolation were never separated by more than two millimicrons.

Since the animals had already been trained to choose the more red of two stimuli, testing was begun in the red end of the spectrum. Limens were then measured at intervals passing down through the spectrum to the violet end. Because of the high limens in the red end of the spectrum, measurements were made at intervals of 20 mu. In the remainder of the spectrum the limens were spaced by 10 mu steps. Because of the gradual color change in passing down through the spectrum, tests could be made at successive points without additional training. Down to and including 510 mu the variable color was of shorter wave-length than the fixed color. At and below 500 mu the variable color was of longer wave-length. In all cases the longer wave-length color was the positive stimulus whether variable or fixed.

Brightness controls were carried out by varying the relative brightnesses of the two colors from trial to trial. Cues from pattern effects in the lights were controlled by alternately taking the positive color from one monochromator for five trials, and then from the other for an equal number of trials. This control also compensated for any slight, undetected differences in calibration of the two monochromators.

It was, unfortunately, impractical to measure all limens at the same brightness level, since the spectral lights in the blue and violet regions were quite dim. In Fig. 1

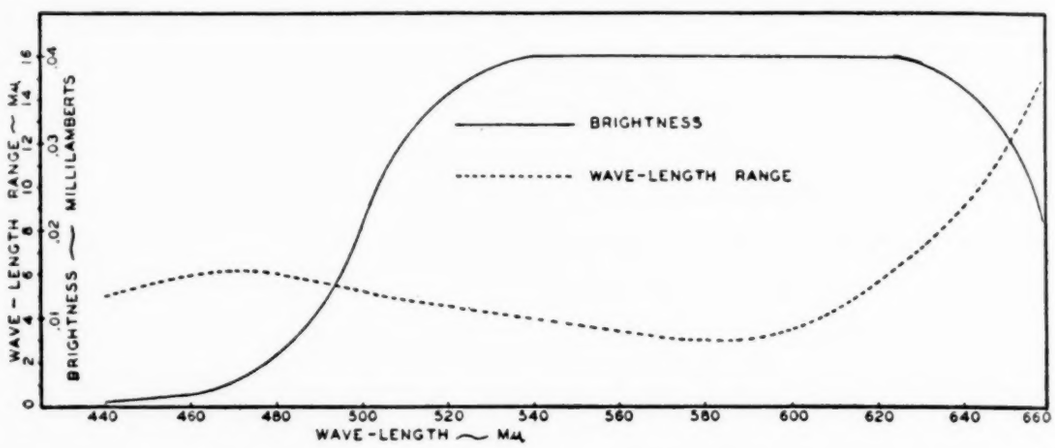

FIG. 1. Curves of brightness (for photopic eye) and wave-length range of spectral lights used in determination of hue discrimination curve.

is shown the variation in average brightness level at which tests were made. These brightness data for the monochromators are based on energy determinations by means of a thermopile. Also, the photometric brightness was measured in the yellow region with a Macbeth Illuminometer. With the aid of visibility data for the average, human, photopic eye it was then possible to convert the energy values into millilamberts. Because of the Purkinje effect under the conditions of darkness at which tests were made, the shorter wave-length colors were relatively brighter than the values given in Fig. 1. However, the photopic brightness is presumably the important value for hue discrimination.

The purity of the spectral colors also varied with wave-length. The range of wave-lengths in the color band, at the brightnesses used, varied as shown in the broken line of Fig. 1. This curve, as that for brightness, represents merely average values, since the brightness controls involved changes in both intensity and purity of the colors.

The animals used in this study were immature, laboratory-born chimpanzees. The sexes and ages to the nearest half year were as follows: Hal, male, 3 years; Tom, male, 5½ years; Beta, female, 7 years. Raisins, or pieces of apple or banana, depending on individual preferences, were used as food rewards.

To provide, for purposes of comparison, a human hue discrimination curve in addition to those of previous investigators a woman (M. G.) was also tested for limens throughout the spectrum. Except for the use of shorter series of wave-length differences, and the use of verbal rewards, these measurements were made in the same manner as those for the animals.

III. Results

The complete results are presented in Fig. 2. The actual limens of the animals are indicated by the circles, triangles, and squares. These points have been plotted on the abscissa at wave-lengths midway between the central wave-lengths of the fixed and variable colors. For example, with the fixed color at 640 mu, a limen of 12 mu was plotted at 634 mu. Drawn through the means of these limens is the solid line representing the unsmoothed, chimpanzee hue discrimination curve.

Also shown in Fig. 2 is the unsmoothed hue discrimination curve for the one human subject. For purposes of comparison the composite hue discrimination curve of Wright and Pitt (1934) for five human subjects is included. Of the several similar curves that could have been used for comparative purposes this one from Wright and Pitt was chosen because it represents the discrimination of several subjects. Also the brightness control appeared somewhat more adequate than in most of the other studies. Except for the absence of a secondary red minimum, this curve from Wright and Pitt resembles those of other investigators.

A check mark drawn in some of the symbols of Fig. 2 indicates that these limens were taken from the data of the earlier study of hue discrimination at three sprectral points. As the experiment progressed a suspicion arose that these limens might have been lower if determined after the additional practice had by the animals. The limens at 500 mu were, therefore, redetermined, and the curve at this point is based upon these later data. As the points show, the discrimination of Beta and Tom was somewhat better on the second measurement, although that of Hal was poorer. At

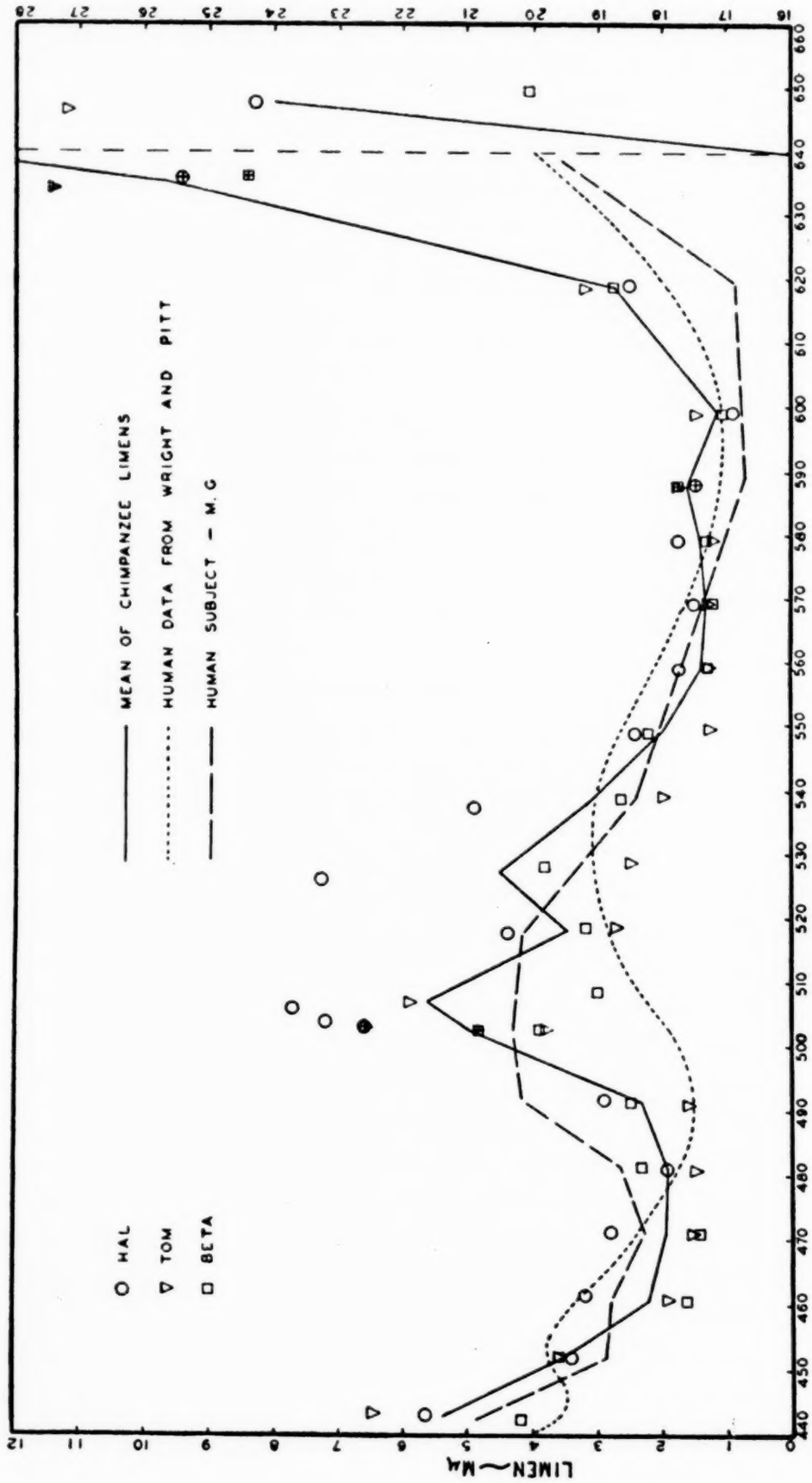

FIG. 2. Hue discrimination curves of chimpanzees and human beings. Solid line, three chimpanzees. Long-dash line, one human subject. Short-dash line, five human observers of Wright and Pitt (1934).

640 mu only the limen of Beta was redetermined and found to be slightly higher. The slight rise in the curve at 589 mu suggests that these limens would have been slightly reduced had they been measured a second time.

Since the limens at the last point in the red region were extremely high they have been plotted on a separate scale marked off at the right side of Fig. 2. The limen for the human subject at this point was 12.3 mu, which was too low to be plotted on this scale.

IV. Discussion of Findings

In discussing the findings of the present study there are two general problems. First, how can the differences between the human hue discrimination curve, as determined by the present method, and the earlier curves be accounted for? Second, what valid differences are there between the hue discrimination curves of chimpanzee and man?

The Effect of Method of Determination upon the Form of the Hue Discrimination Curve

Both the chimpanzee hue discrimination curve and that for the one human subject differ in certain respects from the curves of Wright and Pitt and other investigators. The chief differences are that the central rise and the minimum in the blue-green region are, in the present curves, both shifted toward the shorter wave-lengths. Another difference is the absence of the secondary minimum in the violet region in the present curves.

The author is inclined to attribute the absence of the violet dip to one or more of three experimental conditions. The low brightness of the spectral lights at this end of the spectrum most likely made maximal hue discrimination impossible. The purity of the spectral colors probably was lower than that used by some of the previous investigators. Also, the 10 mu separation between test points may have been too gross to show a minimum as short as that in the violet region.

The shift of the blue-green minimum toward the shorter wave-lengths is somewhat surprising, especially since the

brightness at which the tests were made dropped off rapidly toward this end of the spectrum. Laurens and Hamilton (1923), however, found that hue discrimination is quite constant over a considerable brightness range. It is unlikely that the one human subject, for whom a discrimination curve was determined, was atypical. At 500 mu, one of the crucial points, the limens of two other human subjects tested at three spectral points (in press, *a*) had limens equally high. For the shift in position of the central maximum and the blue-green minimum in this study, as compared to the earlier ones, dissimilarities in experimental method offer a plausible explanation.

The usual method of determining human hue discrimination limens has been to allow the subject to change the wave-length of one of two colors until it differs perceptibly from the other. When the brightnesses are not equated, as in the case of Steindler's (1906) measurements, such a limen does not represent purely hue discrimination. The just noticeable difference may be based largely upon brightness inequalities. This is especially true in the region around 500 mu where the visibility curve has a very steep slope. In the central region and at the two extremes of the spectrum, where brightness varies less with wave-length, such limens are more valid measures of hue discrimination.

In the experiments of König and Dieterici (1892), Laurens and Hamilton (1923), and Wright and Pitt (1934) the subjects themselves equated the brightnesses. Nevertheless, at liminal values it is quite difficult to distinguish between brightness and hue differences, and the spurious influence upon the limen of rapidly changing brightness may not have been avoided entirely by allowing the subject himself to equate the intensities. The close resemblance of the hue discrimination curves of the above investigators to those of Steindler, in respect to the position of the blue-green minimum, supports the foregoing interpretation.

Obviously the psychophysical method in the present experiment differed markedly from that used in the earlier studies with human subjects, and it appears quite probable,

in view of the above discussion, that this methodological difference accounts for the shift in position of the blue-green minimum and the green maximum. The present psychophysical technique was a modified method of constant stimuli. The experimenter rather than the subject controlled the changes in wave-length and brightness. Instead of merely recognizing the existence of a hue difference, the subject was required to judge the direction of the difference by choosing the positive color. Because of the method of controlling the intensity factor, this judgment was usually made with unlike rather than equated brightnesses. Whereas the rapid change in spectral brightness in the blue-green region may have lowered the limens in the earlier studies, it could hardly have had such an effect in the present study.

Whether or not the above interpretation is correct, the present results raise the question as to whether the previously determined human hue discrimination curves are entirely valid. The psychophysical methods used probably were not the most satisfactory. A redetermination by the method of constant stimuli might find curves resembling more closely those of the present study.

Comparison of Chimpanzee and Human Hue Discrimination Curves

In an earlier study the author (in press, *a*) found evidence that chimpanzee hue discrimination is slightly inferior to that of man at the long wave-length end of the spectrum. The present results are consistent with this finding. At the last point at which tests were made, with the fixed color at 660 mu, the limen of the one human subject was only one-half the mean of the chimpanzee limens. At 640 mu the separation between the chimpanzee and human curves is large, and continues in some degree down to a wave-length of about 570 mu. In the shorter wave-length end of the spectrum the chimpanzee curve conforms rather closely to that of the human subject used in this study. The deviations from the curve of Wright and Pitt in the blue and green regions have already been discussed.

The significance of the slight inferiority of chimpanzee hue discrimination in the long wave-length end of the spectrum has already been discussed in relation to color vision evolution in another publication (in press, *a*). The results offered in the present report support the interpretation that the evolutionary development of color vision in these apes is equivalent to that in man except for differentiation of color differences in the long wave-length end of the spectrum. In comparison with dichromatic vision, however, the inferiority of chimpanzee hue discrimination is very slight. It would seem almost certain, from the form of the hue discrimination curve, that the color vision of chimpanzees is trichromatic.

V. Summary and Conclusions

By measuring difference limens at intervals throughout the spectrum, hue discrimination curves were obtained for three chimpanzees and one human subject. Comparisons among these and the curves found by earlier investigators for human subjects permit the following conclusions:

1. The precise form of the human hue discrimination curve varies with the psychophysical method of measuring the limens. It is probable that in the traditional curves the blue-green minimum is displaced toward the longer wave-lengths because of confusion of hue with brightness differences.

2. The chimpanzee hue discrimination curve is essentially like that of man except for higher limens at the long wave-length end of the spectrum.

3. Because of the similarity in form of the hue discrimination curves it is highly probable that chimpanzee color vision is basically like that of man, and hence, trichromatic.

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A TACTILE GENERALIZATION GRADIENT FOR A PSEUDO-CONDITIONED RESPONSE *

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AND

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The purpose of the present study was to investigate the possible existence of a tactile generalization gradient resulting from pseudo-conditioning the galvanic skin response. *Pseudo-conditioning* may be defined as the formation of a set to respond to a formerly inadequate stimulus following a series of presentations of some strong unconditioned stimulus. The unconditioned stimulus is never presented simultaneously or nearly simultaneously with the inadequate or *pseudo-conditioned* stimulus.

In connection with research in conditioning, a number of writers (*cf.* **4**) have reported control experiments in which pseudo-conditioning was evident. Pseudo-conditioning of the galvanic skin response was reported by two of these writers, Switzer (**13**) and Steckle (**12**). In a study of factors affecting the galvanic skin response, Davis (**2**) reported what corresponds to pseudo-conditioning, when he stated that strong stimulation results in an increase in the responsiveness of the skin resistance mechanism to weaker stimuli.

Data bearing on the generalization of pseudo-conditioned responses have been reported in two studies. Harlow and Toltzien (**7**) found that pseudo-conditioned responses were generalized to non-experimental or neutral environments where shocks had not been administered. The strength and

* This experiment was carried out at the University of Wisconsin during the summer of 1939 and was supported in part by a grant from the graduate research fund for 1939–40. The writers wish to thank Dr. Harry F. Harlow for suggestions and criticism during the course of the experiment.

frequency of the pseudo-conditioned responses were not as great in the neutral environment as in the experimental situation. Grant and Meyer (**4**) reported that the frequency of pseudo-conditioned hand withdrawals elicited by a buzzer placed near the shocked hand was greater than the frequency elicited by a buzzer placed at a distance.

The present experiment continues the study of the generalization of pseudo-conditioned responses.

Procedure

The apparatus and general technique were in all respects similar to that reported in a previous study by the present writers (**3**).

The subject was in a room separate from that in which the controlling and recording apparatus was located. Auditory stimuli were eliminated by a constant noise screen, and visual stimuli were minimized by the location of the subject's cot and the placement of a wooden auxiliary screen.

The pseudo-conditioned stimuli were provided by vibro-tactile stimulators of the type described by Bass and Hull (**1**).

The unconditioned stimulus was an interrupted induction shock, the intensity of which was adjusted so that most of the subjects reported it to be unpleasant but not painful. The electrodes were two zinc strips fastened about 5 mm apart on a block of wood.

Presentation of all stimuli was controlled by the pendulum of a Dodge photochronograph. The duration of each vibro-tactile stimulus was 435 msec., and the duration of each shock stimulus was 190 msec.

The galvanic skin response was recorded by the Féré technique. The electrodes and recording galvanometer were the same as those used in the previous study (**3**).

Fifteen male students served as subjects in the present experiment. They ranged in age from 19 to 26, with a median age of 22. Only one of the subjects knew anything of the nature of the experiment. The record of one additional subject was discarded without having been read because one of the vibro-tactile stimuli was accidentally paired with a shock stimulus during a pseudo-conditioning series.

The subject lay prone on a cot. The vibro-tactile stimulators, placed approximately three and one-half to four and one-half inches apart, were glued about two inches to the right of the midline of the back, starting at the shoulder and continuing to the waist. In the case of seven subjects, the shock electrode was taped about one-half inch cephalad of the vibro-tactile stimulator on the shoulder. For the remaining eight subjects, the shock electrode was taped about one-half inch caudad of the waist stimulator.

As in the previous study, alternating series of tactual stimuli and shock stimuli were given. Each *Tactile stimulus series* consisted of two stimulations from each vibro-tactile stimulator at intervals of from 10 to 30 sec. These stimulations were given in balanced permutational orders to eliminate the influence of adaptation. Each *Pseudo-conditioning series* consisted of five single shocks at intervals of from 10 to 30 sec.

The order of presentation was as follows:

I. Tactile stimulus series, pre-pseudo-conditioning.
II. Pseudo-conditioning series.

III. Tactile stimulus series, test.
IV. Pseudo-conditioning series.
V. Tactile stimulus series, test.
VI. Pseudo-conditioning series.
VII. Tactile stimulus series, test.
VIII. Pseudo-conditioning series.
IX. Tactile stimulus series, test.

In all cases, the interval between each pseudo-conditioning series and the following tactile stimulus series was varied at random between 10 and 30 secs. The interval between each tactile stimulus series and the following pseudo-conditioning series was in excess of one minute in the case of three subjects and in excess of two minutes in the case of the remaining twelve subjects.

Results

The mean and probable error of the galvanic skin responses to shock was $10.38 \pm .92$ mm. The unit of all galvanic skin responses reported in the present study is one mm deflection on the scale of the galvanometer.

The vibro-tactile stimulator in close proximity to the point at which the shocks were delivered was numbered 0, with the remaining stimulators numbered 1, 2, and 3 respectively in order of their distance from 0.

In the pre-pseudo-conditioning trials, before any shocks had been given, the means and probable errors of the skin responses to each vibro-tactile stimulus were as follows:

	Point Stimulated			
	0	1	2	3
M. ± P.E.	$3.85 \pm .66$	$3.51 \pm .57$	$4.09 \pm .63$	$2.80 \pm .60$

The probable errors given above were calculated from the complete distributions of the raw data. The critical ratios (mean differences divided by the standard errors of the mean differences) between the pre-pseudo-conditioning responses to the stimulators taken pair by pair are presented below.

	1	2	3
0	.50	− .37	2.39
1		−1.33	1.27
2			2.98

A positive critical ratio indicates that the difference favored stimulation nearer point 0, and a negative critical ratio indicates the difference favored stimulation nearer point 3. Thus, the critical ratio of the difference between the amplitude

of the responses to point 0 and point 2 is .37 in favor of point 2. The standard errors used in the critical ratios were computed from distributions of *mean differences* (*i.e.*, each subject accounted for a single entry in the distribution of differences between point 0 and point 3).

The difference between the mean deflections of point 0 and point 3 is not statistically significant (CR = 2.39). This difference may very well be due to the fact that the subject knew from the placement of the electrodes and the experimenter's explanation that the shock was to be administered near point 0. With this knowledge, stimulation of any point on the back would quite likely cause a response, unless the difference between the stimulated point and point 0 was clearly discriminated, even before shocks were administered.

The mean galvanic skin response of each subject to each stimulator is presented in Table 1. These averages include

TABLE 1

PSEUDO-CONDITIONED GALVANIC SKIN RESPONSES TO DIFFERENTIALLY PLACED VIBRO-TACTILE STIMULI *

Subject	Point Stimulated			
	0	1	2	3
1	2.13	2.06	1.50	1.56
2	1.94	1.69	1.25	0.56
3	4.38	3.00	3.38	3.50
4	1.69	1.38	1.75	1.38
5	1.63	1.38	0.63	0.81
6	2.88	2.00	3.56	2.13
7	2.38	2.88	2.94	2.38
8	11.13	12.50	12.75	8.63
9	3.63	3.19	2.25	1.44
10	4.19	3.81	3.88	3.31
11	2.38	1.94	1.25	1.81
12	3.94	5.19	2.69	3.13
13	4.88	2.56	5.50	2.44
14	0.56	0.56	0.75	0.75
15	2.06	2.56	1.63	1.06
Mean±P.E.	3.33±.55	3.12±.65	3.04±.59	2.34±.48

* Each column lists the means of the galvanic skin responses of the individual subjects for the point stimulated. Column 0 designates the point of stimulation which was in close proximity to the point at which the shock electrodes were placed. Columns 1, 2, and 3 represent points of vibro-tactile stimulation which were placed at successively distant intervals from point 0. The means and probable errors are based on the raw data summarized in each column.

the data of all trials after the initial pseudo-conditioning series. The usual variability (**1, 4, 8, 9, 10, 11**) in the gradients of individual subjects is to be found in this table.

The means and probable errors summarizing the galvanic skin responses of all subjects for each of the tactile stimuli appear at the bottoms of their respective columns. The critical ratios between the pseudo-conditioned responses to the stimulators taken pair by pair were as follows:

	1	2	3
0	.88	1.25	4.80
1		.20	2.79
2			2.26

The mean deflections are also plotted graphically in Fig. 1, mean galvanic skin response against stimulus placement. The data give a gradient of decreasing response-magnitude

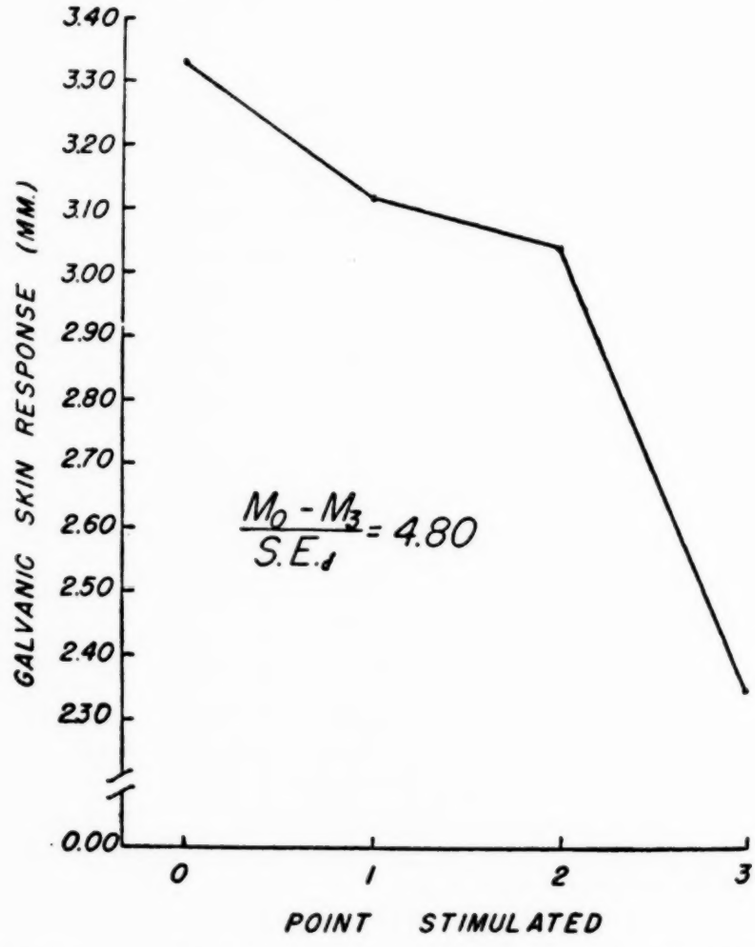

FIG. 1. Tactile generalization gradient resulting from pseudo-conditioning the galvanic skin response.

showing no inversions. The critical ratio of the gradient $\left(\frac{M_0 - M_3}{\text{S.E.}_d} \text{ in Fig. 1}\right)$ was computed from the distribution of the individual differences between the mean deflection to stimulator 0 and the mean deflection to stimulator 3, as in the previous study (**3**). The critical ratio of the difference between the mean response to 0 and the mean response to 3 was 4.80 after pseudo-conditioning, as contrasted with the critical ratio of 2.39 before pseudo-conditioning. A critical ratio of 4.80 signifies that such a difference would be found in less than 1 in 10,000 random samples if a true difference did not exist. However, the size of the sample used necessitates a more conservative interpretation. Although the mean response elicited by stimulating point 2 exceeded the mean response to stimulation at point 0 before pseudo-conditioning, after pseudo-conditioning the difference favored 0 with a critical ratio of 1.26.

Discussion

The average galvanic skin response to stimulation at each point is less after the pseudo-conditioning than in the pre-pseudo-conditioning trials. This fact is not unusual, however, having appeared in the conditioning studies of Bass and Hull (**1**), Hovland (**8**, **9**, **10**, **11**), and the present writers (**3**). The lower amplitude of reaction is due to the rapid adaptation of the galvanic skin response (*cf.* Davis, **2**).

The averaged results of the present experiment give a clear-cut generalization gradient for the pseudo-conditioned response tendency. Davis (**2**) attempted to explain the increased galvanic skin responsiveness to weaker stimulation following strong stimuli in terms of a lowering in the amount of polarization at the electrodes caused by the skin reaction to the strong stimuli. This lower polarization then permitted the response to weaker stimuli to result in a greater flow of current through the electrodes and hence to indicate a seemingly larger shift in skin resistance. Since the location of the galvanic skin electrode and hence the site of depolarization was the same for all four of the stimulators, this explanation

cannot account for the appearance of the gradient in the present study. The gradient must be explained in terms of some kind of central process, whether that process involves hyperexcitability of the response centers due to a general increase in emotionality or whether it involves some kind of discriminatory response set which is not subject to verbalization. If a general increase in emotionality is a partial basis for an explanation, the clear-cut polarity of the gradient remains to be accounted for in some other way. A discriminatory process is definitely involved.

From another viewpoint, the results of the present experiment might be seen as involving conditioning by spatial contiguity rather than by the usual temporal contiguity. The fact that the pseudo-conditioning gradient is more reliable than the conditioning gradient of the previous study (**3**) may be due, in part, to the fact that the spatial relations of the stimulus conditions (discrimination of which is mirrored in the gradients) are laid down by the locus of the unconditioned stimulation and need not be conditioned by repeated paired reinforcements. That is, *the 'reinforced' pseudo-conditioned stimulator acquires its greater excitatory power by virtue of the perception of already existing and easily discriminable spatial relationships between the loci of the pseudo-conditioned and unconditioned stimuli, whereas the corresponding excitatory or signalling power of the reinforced stimulator in the ordinary conditioning experiment must be acquired through the somewhat less efficient process of temporal conditioning.*

The generalization gradient for pseudo-conditioning demonstrated by the present study gives evidence to be considered in the elaboration of any theory of the mechanisms of conditioning. Switzer (**13**) has shown that the increased responsiveness to formerly neutral stimuli following strong stimulation (*i.e.*, pseudo-conditioning) is subject to *disinhibition*—one of Pavlov's traditional phenomena of conditioning. The data of Switzer and of the present study seem to be in harmony with the suggestion of Grant and Meyer (**4**) that several of the traditional conditioning phenomena may be functions primarily of the *set to respond to the unconditioned*

stimulus, and are not due primarily to the *association* of the conditioned and unconditioned stimuli which occurs during the reinforcement trials of conditioning.

Evidently, after a few presentations of a strong stimulus, a set to respond to that stimulus is formed. The set to respond is generalized in such a fashion that almost any stimulus of sufficient intensity will elicit the response. This would account for the initial generalization of the conditioned response. Subsequent differentiation is due to association and, in view of the gradient in this study, contiguity in space as well as in time may have some influence in setting up the differentiation.

Summary

Average galvanic skin responses of fifteen subjects to vibro-tactile stimulators placed along the back gave a clear-cut gradient of response amplitude after the presentation of a number of electric shocks. The gradient sloped away from the point at which the shocks were delivered.

This generalization gradient must be explained in terms of some persisting central state. The polarity of the pseudo-conditioned gradient involved might be explained as the result of conditioning by spatial instead of the usual temporal contiguity.

The appearance of a generalized response gradient without associative conditioning implies that such gradients may not be primarily the result of the paired stimulation during conditioning, although the polarity of a response gradient can be established by paired reinforcement.

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FACILITATION OF THE ALPHA RHYTHM OF THE ELECTROENCEPHALOGRAM

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In attempting to discover psychological correlates of the electroencephalogram (EEG) attention has been directed for the most part to the blocking of the occipital alpha rhythm. Experiments on many different phases of the phenomenon have been performed. It has been found in general that sensory stimulation is effective in blocking the rhythm, stimulation of some modalities proving more effective than stimulation of others. In addition to sensory stimulation it has been shown that attention is involved in the blocking process, and that blocking does not occur unless some degree of attention to the stimulus is present. Stimuli with the greatest attention or 'arousal' value were found to be most effective in depressing the alpha rhythm.

Other psychological correlates of the EEG have been obtained. For example, it is agreed that anxiety or other emotional disturbances increase the frequency of, and sometimes disrupt the alpha rhythm (**4**). Hoagland *et al* (**3**) have shown a relationship between emotional states and the slow wave activity in the EEG, while Oberman (**7**) found a relationship between alterations in the alpha rhythm and mild affective states.

In 1937 Forbes and Andrews (**2**) attempted to relate the blocking of the alpha rhythm and the psychogalvanic reflex, which has many psychological correlates, but without success.

Loomis, Harvey and Hobart (**5**) have reported an effect of attention and stimulation on the alpha rhythm which is the exact opposite of the usual blocking. They found that the intent listening for a barely audible auditory stimulus facilitated or increased the amount of alpha rhythm and never

decreased it as compared with any other section of record. Likewise Bagchi (1) found that the alpha rhythm was often facilitated (increased) upon reappearing after having been blocked by an auditory stimulus (spoken word). Similar facilitation observed following the blocking of the rhythm by a light stimulus has been reported (4).

Facilitation of the alpha rhythm during sleep has been reported by Loomis, Harvey and Hobart (6). They found that auditory stimuli with 'arousal' value, the same type that arrests the rhythm when the subject is awake, caused the alpha waves to appear briefly without the sleeper being awakened.

Aside from these observations the facilitation effect has been largely neglected. The results of this study suggest, however, that it is closely related to the psychological state of the subject in a manner similar to the psychogalvanic reflex (PGR) (8). While not so sensitive as the PGR, facilitation occurs as a response to the same general type of stimuli except where the stimuli are such as to cause blocking instead of facilitation.

Apparatus and Procedure

The amplifying system * used for recording the EEG consisted of two channels of matched high gain resistance capacity-coupled amplifiers. These actuated dynamic ink writing recorders. A sensitivity of about 5 microvolts per millimeter deflection was used. Two records were obtained simultaneously on a paper tape moving at an adjustable rate of speed. One second time intervals were recorded directly on the tape by a third pen. A fourth pen was used to record the presentation of stimuli to the subject. Signals from the subject were recorded on the tape by the time pen.

The electrodes consisted of small solder buttons fastened to the scalp with collodion. Transoccipital leads were used throughout. Occasionally simultaneous records were taken from the precentral area.

The subject was placed on a bed in an electrically shielded semi-sound-proof room. Since facilitation was being studied, records were taken when the subject's eyes were open. A dim light was present in the room. This was done because the amount of alpha activity in records taken under such conditions is low. Hence facilitation of the rhythm could be easily observed. Facilitation occurring in a record where the alpha activity is already abundant (eyes closed) is difficult to detect and may often be overlooked.

Seven psychologists and nurses at the New York State Psychiatric Institute and Hospital were used for subjects. The majority of the experiments were carried out on three of them. Four others served as controls.

* The amplifying system was designed and constructed by Mr. Walter E. Rahm, Jr.

Results

Under the conditions described above, facilitation, when it occurred, usually appeared as a burst of regular, high amplitude alpha waves which stood out clearly from the level of alpha activity in the control portion of the record. Several bursts often occurred and sometimes the facilitation effect lasted for longer than a minute. Occasionally facilitation could not be detected by direct inspection in which case it was necessary to measure the percent time alpha of the portion of record in question and compare it with the percent time alpha of the control section.

The stimulus situations which thus far have been found to elicit facilitation may, with one possible exception, be included under a single descriptive category. The most general description would characterize them as stimuli causing a sudden mobilization of the subject's reactive apparatus. That is, the induction in the subject of general states of readiness, attentiveness or heightened awareness was usually accompanied by the facilitation of the alpha rhythm.

It was found that if a state of readiness were induced in the subject facilitation often occurred. A state of readiness was induced by presenting a ready signal indicating to the subject he would be required in a second or two to perform some task. Two principal tasks were used. The first consisted of the solution of mental arithmetic problems. The second was a kind of multiple choice task which consisted of a series of two stimuli, a flash of light and a buzzer sound, presented in rapid sequence and random order. The subject was required to signal differentially by pulling a string once for the buzzer, twice for the light and not at all if both were presented simultaneously. The ready signal consisted of the experimenter calling 'ready' a few seconds before the presentation of the task. The ready signal was usually followed by a burst of alpha waves occurring before the task itself was presented.

Other experiments showed that a stimulus which ordinarily did not elicit facilitation could be made to do so when

used as a ready signal. For example, a bell previously unable to induce facilitation was sounded each time before the subject was asked to solve a simple arithmetic problem. After four such trials the bell was followed by a small but recognizable burst of alpha waves which came before the problem was given. Two presentations of the bell without the problem produced experimental extinction. In most of these experiments the subject was not told beforehand that the bell would be followed by the presentation of a problem. When this was told to the subject beforehand the facilitation following the bell was more marked and occurred after the first trial.

It was found that some stimuli were capable of eliciting facilitation by virtue of their ability to catch and hold the subject's attention. The states induced by these stimuli were hence called states of awareness or attentiveness. Thus facilitation could be induced merely by questioning the subject. Such questions as "When is your birthday?" or "Do you think you could go to sleep?" were often followed by bursts of alpha. But if the experimenter merely made statements, not addressed to the subject in particular, facilitation almost always failed to take place. Questions of a personal nature were usually more effective than neutral questions in producing facilitation. For example, one subject was asked a question which made her laugh. On the record, after the movement artifacts due to laughter had stopped, the percent time alpha was near 100 for the next 30 or 40 seconds while more questions were asked. The experimenter during this period was in the shielded room sitting beside the subject. When the questions were over and the experimenter left the room the amount of alpha diminished rapidly. Two minutes later it returned to the control level of 5 to 10 percent.

Sudden commands to the subject to perform simple acts such as picking something up or placing it down were often followed by facilitation. General instructions as to procedure etc. also had their effect on the alpha rhythm. Following the giving of instructions to subjects the percent time alpha was

invariably higher than before instructions were given. At times the increase amounted to 2 or 3 times the original control percent time alpha.

A different kind of stimulus situation, which was noted above as an exception, was also successful in producing facilitation. Relaxation following the completion of a task was usually followed by large well-defined bursts of alpha rhythm. Mental arithmetic problems and the multiple choice task were most successful in this respect. For all tasks the subjects were requested to work hard and as rapidly as possible and to relax completely when the task was completed. The end of the multiple choice task was indicated by an 'over' signal from the experimenter. The facilitation appeared as bursts of high amplitude waves. The bursts occurred from $\frac{1}{2}$ to 15 seconds following the completion of the task. Sometimes only one burst occurred, but more often two or three. Occasionally the facilitation effect lasted for two minutes following the completion of a task. There seemed to be no relationship between the difficulty of the task and the size and duration of the bursts.

In order to illustrate how the amount of alpha activity was influenced by changes in the general psychological state of the subject, a protocol of a single experimental session with one subject will be summarized as follows:

Subject: M. L.
Percent time alpha eyes closed = 69
Experimental session (eyes open throughout)

1. Subject rests relaxed with eyes open. Control record—percent time alpha = 7.8.
2. Without warning, S given mental arithmetic problem. Solution followed 10 seconds later by large burst of alpha. Percent time alpha for 60 seconds following solution = 21.
3. Period of rest (5 minutes)—no record taken. Instructions given for multiple choice experiment.
4. Immediately following instructions, percent time alpha = 27. Ready signal for multiple choice experiment

followed by large burst of alpha. After task, for 60 seconds percent time alpha = 49. Large bursts occur. This effect lasts for longer than two minutes when record was stopped.

5. Rest period—no record taken. S smokes and talks.
6. Control record—no instructions. Percent time alpha = 18.5.
7. Instructions given for multiple choice task. Following instructions (60 seconds) percent time alpha = 41. Following task percent time alpha = 54.
8. Rest period with eyes closed (3 minutes).
9. Control record (eyes open)—no instructions—percent time alpha = 13.
10. S given instructions to subtract consecutive 7's from 100. Ready signal followed by burst of alpha. Percent time alpha following solution = 42.

Examples of the facilitation effect may be seen in Fig. 1.

A Special Case of Facilitation

A series of experiments were performed on a subject who had no alpha rhythm in her record even when her eyes were closed. The experiments described above were performed in an attempt to bring out the alpha rhythm by facilitation; but with no success. However, it was found that while the subject was asleep the alpha rhythm could be elicited following stimulation. After the subject had been asleep for two hours, stimuli such as a bell, the rustling of paper, or calling the subject's name, were followed by well-defined bursts of alpha. The stimuli did not wake the subject. Natural movement in sleep was also followed by the appearance of the alpha rhythm. The sleep record was unlike those reported by other observers, being almost flat, without spindles or slow waves.

Some Characteristics of Facilitation

It was found that the facilitation effect could undergo a process of adaptation. For example, when the multiple choice task was repeated at intervals of 5 or 6 minutes the

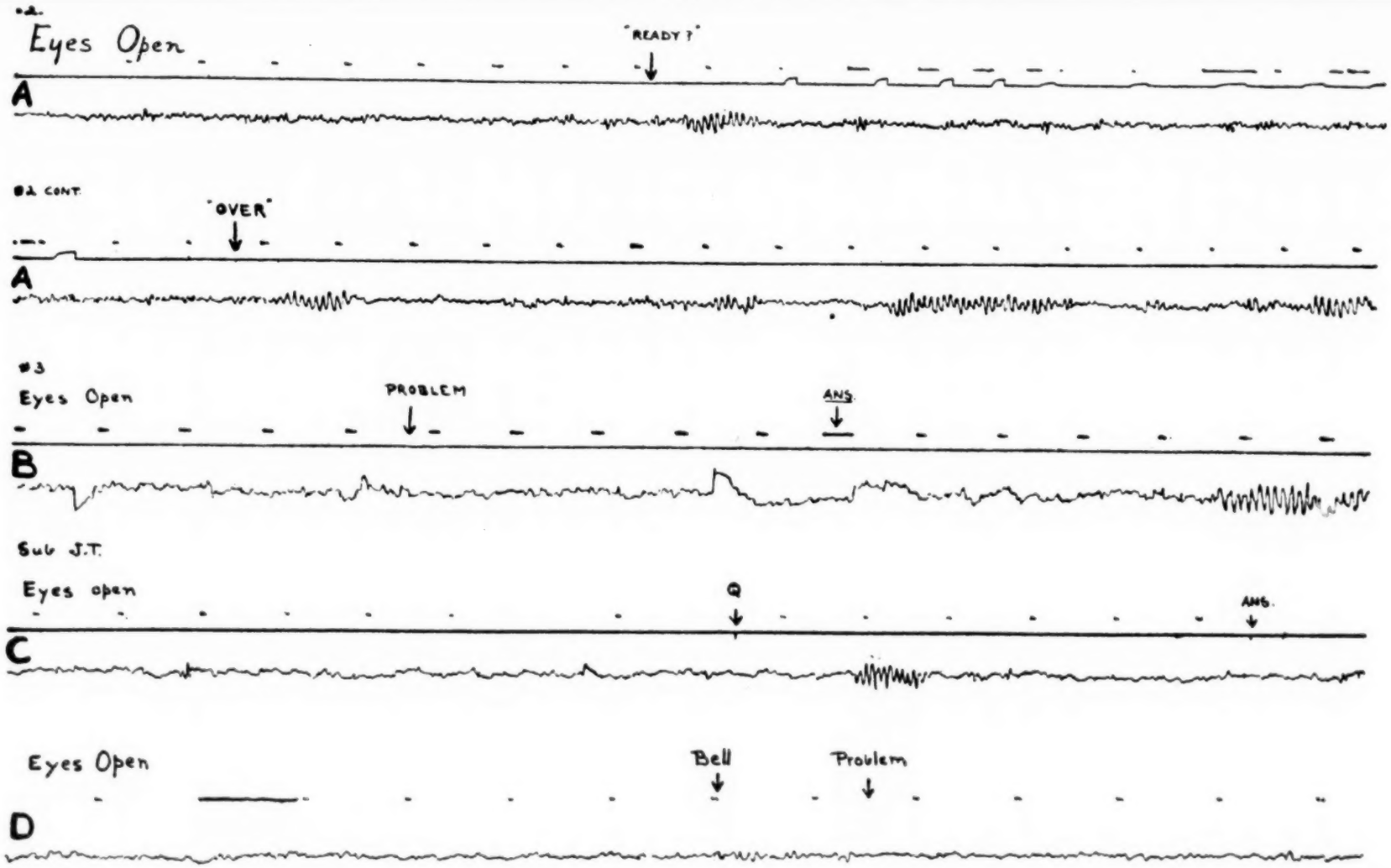

FIG. 1. Facilitation of the alpha rhythm. *A* = Facilitation following ready signal for multiple choice task and again following the completion of the task. Top line = seconds; middle line = presentation of stimuli. *B* = Facilitation following the completion of a mental arithmetic problem. *C* = Facilitation following asking subject a question. *D* = Facilitation following using bell as a ready signal.

facilitation effect following it became progressively less. Similar findings were obtained upon the repetition of the problem solving tasks. The adaptation of the facilitation effect was observed following the second repetition in some instances.

An analogous phenomenon was observed during the progress of a learning experiment. Mirror drawing was used as the task. The subject was required to trace as quickly as possible one-half a standard star for each trial. Six consecutive trials were used. Following each of the first four trials facilitation of the alpha rhythm occurred. Following the last two trials no facilitation occurred. According to the reports of the subject the last two trials were not difficult since the knack of tracing had been learned by that time.

Facilitation was found not to be limited to the particular level of excitation represented by the eyes open condition. Other investigations have reported it occurring during sleep and when the eyes are closed. Therefore, further experiments were performed showing that the alpha rhythm may be facilitated while the subject is engaged in reading a book. Reading represents presumably a higher level of excitation than the eyes open condition alone. Ready signals, and the tasks described above were used. Immediately following the solution the subject returned to reading. The facilitation occurred while the subject was reading.

The independence of the occipital and pre-central alpha rhythms could be easily seen during facilitation. For example, during a session where the subject was being asked questions, bursts of facilitated alpha potentials occurred on and off throughout the period from both leads. Bursts appeared more often in the occipital than in the pre-central lead. Occasionally, however, bursts appeared in the pre-central lead without at the same time appearing in the occipital lead. More often the bursts appeared in both leads simultaneously. In all cases of simultaneous occurrence the facilitation was less marked in the pre-central than in the occipital leads.

The incidence of facilitation was difficult to determine. It did not occur every trial. Furthermore in some subjects

it was easier to elicit than in others. With 'good' subjects in these experiments it occurred with most regularity (about 9 out of 10 times) following the completion of the multiple choice task. Facilitation was less certain to occur following the solution of a mental arithmetic problem. Questions asked of the subjects were the least reliable stimuli.

Comment

The stimulus situations described here which elicit facilitation are similar to those reported as eliciting the PGR. However, without a critical analysis of simultaneous EEG and PGR records taken during situations when facilitation is elicited it is impossible to know if the adequate or essential stimulus is the same for both phenomena or whether, in fact, there may be two different essential stimuli, one for facilitation and one for the PGR. Until such an analysis is made there is not sufficient evidence for equating the two responses in terms of their underlying mechanism.

The question arises as to the difference between stimuli which block the alpha rhythm and those which facilitate it. Under different conditions, the same stimulus can often be made to do either. Furthermore, both responses can be made to occur in succession following a single stimulus. For example, a light stimulus may cause blocking, but when the block is over the alpha rhythm often returns with increased amplitude and regularity for a short burst compared with its appearance before the stimulus was presented. Finally, it is known that the physical properties (intensity and duration) of the stimulus are important to the blocking response whereas they are unimportant for facilitation. This suggests that blocking and facilitation are symptoms of two different types of reaction and that the same stimulus may contain the essential stimuli for each type. Further delineation of the types awaits further experimentation.

Summary

Facilitation of the occipital and pre-central alpha rhythms was described as an increase in amplitude and regularity of

the potentials, usually appearing as a series of bursts. The experimental records were taken while the subjects' eyes were open so that facilitation could be more easily detected.

Stimuli eliciting facilitation were found to be similar to those reported as eliciting the PGR. They were characterized by a change in the general psychological state of the subject from relaxation to readiness, attentiveness, or awareness, and from these states back to relaxation. The states were induced by putting sudden questions to the subjects, giving instructions, presenting ready signals indicating the beginning of some task, producing relaxation following the completion of a task, etc.

Facilitation was found to undergo a process of adaptation. Individual differences in the ease and regularity of eliciting facilitation were described.

It was found that the facilitation of the occipital and precentral rhythms could at times be elicited independently.

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THE EFFECT OF BENZEDRINE SULFATE ON SYLLOGISTIC REASONING [1]

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Introduction

Statement of the Problem.—The effect of drugs on the higher thought processes was first studied by Kraepelin in 1883 (9, p. 170). However, the effects of the drug benzedrine sulfate are of much more recent interest, the first observations on the compound having been published in 1930 by Piness, Miller, and Alles (10). Benzedrine sulfate, since that time, has been used in many experiments, giving rise to some interesting findings and some more interesting speculations.

Most of the papers published on benzedrine indicate its stimulating effect upon the central nervous system. Thus, the compound lends itself to application in experimentation on the higher thought processes. The speculations concerning the effects of benzedrine on 'mental power' have led to this present attempt at a systematic analysis and objective evaluation of these speculations. The problem investigated here is stated in the title of this article. . . .

Reasoning was used as the dependent variable in this investigation because of its accepted higher level of mental function. The syllogistic form was employed because scores on this type of test could be objectively measured and evaluated. Qualities involved in this level of higher mental function have been well stated by Woodworth (15):

> The reasoner needs a clear and steady eye in order to see the conclusion that is implicated in the premises. Without this he falls into confusion and fallacy or fails, with the premises both before him, to get the conclusion. The 'clear and

[1] The writer wishes to express his extreme appreciation and indebtedness to Professor J. P. Guilford for his many suggestions and untiring aid throughout this experiment, and to Professor H. G. Holck for his worthy assistance, and to the Department of Physiology and Pharmacology College of Pharmacy, of the Univ. of Nebraska for the pharmaceutical materials employed in this study.

steady mental eye' in less figurative language means the ability to check hasty responses to either premise alone or to the extraneous features of the situation so as to insure that unitary response to the combination of premises which constitutes the perceptive act of influence.[2]

In the light of this description by Woodworth, it was hoped rather than anticipated that the results of this type of investigation may throw more light on the psychology of the higher mental processes. It is realized that this study is merely one stepping-stone toward greater knowledge in the realm mentioned. It is deemed necessary that, for a systematic knowledge of this subject, many such investigations should be carried out involving several other drugs of known pharmacological action. Also, many more investigations are necessary on different materials in reasoning besides the syllogistic form.

For related studies, the reader may be referred to reviews of the literature on the subject of benzedrine sulfate in psychological experimentation.[3]

Materials

The experiment was conducted in a quiet and secluded room. The benzedrine sulfate was in capsule form containing 10 mg. of the active drug. The control capsule was the same size and shape as the drug capsule, but containing only lactose powder. The capsules had been made in this form for experimental work by the Smith, Kline, and French Pharmaceutical Co., Philadelphia, Penn., and they were furnished to the writer by the Department of Physiology and Pharmacology, College of Pharmacy, University of Nebraska.

A tycos type sphygmomanometer and a stethoscope with interchangeable diaphrams were employed.

The syllogisms used in this investigation were partly taken from those used by Wilkins in a study on reasoning reported in 1928 (14). These syllogisms were of four types, of twenty syllogisms of each type. One type employed normal terms experienced in daily life. A second type employed letters of the alphabet as terms. The third type was made up of pseudo-scientific terms such as 'lavauxia,' 'œnothera,' and 'onagrads.' The fourth type was made up in a suggestible form. The form was such that the stated conclusions were misleading, in that some conclusions which actually are correct in daily life could not formally be drawn from the premises given.

[2] R. S. Woodworth, *Psychology*, 1921, cited by M. C. Wilkins, The effect of changed material on ability to do formal syllogistic reasoning, *Arch. Psychol.*, 1928, **16**, 1–83.

[3] Present status of benzedrine sulfate, *J. A. M. A.*, 1937, **109**, 2064–2069 (A report from the Council on Pharmacy and Chemistry, P. N. Leech, secretary). Brooks, N., The clinical value of benzedrine, a review of literature, *J. Wayne Univ. College of Med.*, 1937, **1**, 9. Also, N. W. Shock, Some psychophysiological relations, *Psychol. Bull.*, 1939, **6**, 447–476.

Another set of syllogisms was constructed by the writer. This was based on the set just described. This new set consisted of the same logical forms as the described set. Consequently, the scoring of the conclusions of a syllogism of the old form would be exactly the same as the scoring of the conclusions of the equated syllogism in the new form . . . a device which also shortened the time necessary in correcting the data sheets, and the two sets were of approximately the same difficulty.

The typewritten instructions for the reasoning test read as follows:

Instructions

The test you are to be given is not an intelligence test but a test of a very special ability. In this test you will be shown cards on which are certain statements followed by certain conclusions drawn from them. You are not concerned with the actual truth or falsity of these conclusions, but you are to determine whether, given these statements, you can draw the conclusions from them. In each case there are three conclusions given, marked *a*, *b*, and *c*. You will be given a separate sheet of paper which is marked with the letters for every card shown. You are to put a minus sign after the corresponding letter for every conclusion that does not necessarily follow from the given statements. Put a plus sign after the corresponding letter for every conclusion that does logically follow from the information given in the statements. Thus, if no conclusion stated can actually be drawn from the given statements, you should place a minus sign after each of the conclusions. The following example is correctly marked:

All a is b; no b is c;
therefore
a. No c is a.
b. Some a is c.
c. All c is a.
(On data sheet:)
a. × b. − c. −

These marks would then be placed after the corresponding letters on your separate sheet for this particular problem. Time will be taken on each card . . . from the time the card is first shown until all the conclusions are marked by you. Accuracy, however, counts as much as speed, but you are to work as quickly as you can.

Mood and fatigue rating scales were adapted from similar material employed by Dysinger (4) in an experimental study of mood.

The Ss used were naive volunteers from the classes in elementary psychology at the University of Nebraska. Of the 20 Ss, 10 were male and 10 were female.

All Ss were required to pass a rigid health examination before they were used as observers in the experiment. This examination was administered for the writer by the Student Health Department of the university. The main purpose of the health examination was to avoid administering benzedrine sulfate to any person whose heart was not in good condition.

Experimental Procedure

Each S chose a particular hour of the week which was kept as constant as was possible for his 4 experimental sittings. It was made mandatory that on the experimental days the Ss were not to drink any coffee, tea, cocoa, coca cola, or any other stimulating drink. This was introduced as a control factor in order to try to get

unadulterated effects from the benzedrine sulfate and also not to get any stimulation effects that were unaccounted for on the days when only the lactose was given to the S.

In the case of the girls used as subjects for the investigation, the menstrual periods of each was ascertained by the doctor who gave the physical examinations. At that time the girls were told not to submit to the tests at any time during a menstrual period, but rather to offer some excuse for those days, such as having to work, etc. This was introduced as a necessary control to avoid any possible ill effects of benzedrine sulfate at that time, also to avoid measuring mental work under such varying conditions as these would have given.

Each S was used for four experimental sittings. The order of administration of the drug and control was counter-balanced for the purpose of controlling factors of training on the tests.

At the beginning of the hour for each sitting, the S was interrogated as to whether or not he had followed the directions given him for that day in regard to refraining from eating between meals and refraining from drinking any stimulating drinks. No S was used on a day during which he reported having not followed the instructions. The S was then given the capsule scheduled for him for that particular day. A half-hour time interval was allowed before taking any mental measurements.[4] After this interval, the S was given the syllogism test. After completing this test he rated himself as to mood and fatigue on the scales mentioned. Then the blood pressure and heart-rate of the S were taken. The procedure as related was performed as precisely the same for each experimental sitting and for each individual as was possible. Each S was continually impressed with the necessity of carrying out his instructions to the letter.

Further information gained about each S consisted of age in months, weight in kilograms, experience in logic, whether individual smoked, his drinking habits, and his intelligence scores. These intelligence scores were taken from the results of the First Nebraska Revision of the Army Alpha Examination.

Results

For every experimental sitting of each individual there was obtained an average accuracy score, an average time score (time taken in seconds for each syllogism), and an average 'power' score. This power score was computed by the formula:

$$\text{Power Score} = \frac{\text{Accuracy Score}}{\text{Time Score}} \times 100.$$

Thus, this power score was a type of efficiency score, measuring the number of correct conclusions made per unit of time. These three scores were computed for each individual for each of the two experimental conditions, drug and control.

Reliability.—The reliability of the test was computed and the corrected (Spearman-Brown formula) coefficients of

[4] A time interval of 30 min. was used as advised in a personal correspondence to the writer by T. B. Wallace of the Smith, Kline, and French Laboratories, Philadelphia, Pa.

reliability for the three scores for the control condition were: accuracy .91, time .87, power .75.

Influence of Benzedrine on Scores for Syllogism Test.—A comparison was made of the average scores under the drug condition to the average scores under the control condition. The separate average accuracy scores for each individual under the drug condition were each compared to the separate average accuracy scores under the control condition. This gave a distribution of score differences for the 20 Ss. The mean of this distribution of differences was found to be only .023, and the standard error of this mean was .031. The difference was in the favor of the drug condition although it was very small. The critical ratio of this difference (for accuracy) was found to be .74, showing that the difference could easily have been due to chance.

The same test was applied to the time and power score differences. In the case of the time scores, the average difference was .38 second (the drug condition having the smaller average time score). The sigma of this mean difference was .68, giving a critical ratio of only .57. This difference is also insignificant. For the power scores the mean difference was .16 in favor of the benzedrine condition, with a sigma of the mean of .29. The critical ratio in this case was an insignificant .56. In other words, the standard errors of the mean differences were in these three cases so large in comparison to the mean differences themselves that the latter were 'completely devoured' by their sigmas.

Statistical Note.—In order to fractionate the data, as was first planned, according to sex, weight, intelligence score, etc., the N would be decreased to one-half. For 20 Ss, in order to be significant, a critical ratio would have to be at least 2.1. However, when only 10 Ss are analyzed, the critical ratio, in order to be significant, must be 2.26 or above (Fisher's test of significance).[5] Thus, fractionations of the data appeared to be a sterile procedure.

This was tested by actually performing some of the proposed fractionations, and the predicted results did appear, namely, no significant differences found.

[5] J. P. Guilford, *Psychometric Methods*, 1936, p. 61f.

Mood and Fatigue.—A comparison of the average mood ratings of the subjects after taking benzedrine sulfate was made with their average mood ratings after taking the control capsules. The mean difference was found to be 2.98 millimeters between the two experimental conditions in the direction of the benzedrine condition. This difference, however, was also found to be quite unreliable, having a critical ratio of 1.1. The mean difference for fatigue between the two conditions was .82, with a critical ratio of .65, showing very unreliable results. Due to the insignificance of these differences in mood ratings and in fatigue ratings, no comparison was made here with the score differences on the syllogism test.

Heart Rate and Blood Pressure.—The mean difference between the two experimental conditions in terms of heart rate was found to be a 1.3 rise in rate under the influence of benzedrine, which rendered a critical ratio of only .55, and so the difference was insignificant.

In the case of the systolic blood pressure, the mean difference between the measures taken after benzedrine and the measures taken after the control was found to 1.9 in favor of the benzedrine condition, with a critical ratio of 2.2. This difference, being above the criterion of significance, was considered as fairly reliable, there being only 13 chances in 1000 that this difference found was due to chance factors alone.

Fractionation as to Blood Pressure Differences.—In view of the significance in results on blood pressure, the data were fractionated on this basis. The scores were taken from those subjects who had exhibited a definite rise in systolic blood pressure due to the effect of the drug. The differences between these two selected groups in terms of accuracy, time, and power scores were respectively .06, 1.03, and .57, all favoring the benzedrine condition.

The critical ratio of the difference found in accuracy for the Ss as a group had been found to be .74, while the critical ratio of the difference in accuracy for those subjects whose systolic blood pressure was raised by benzedrine was 1.5. The critical ratio for the time score was .57, while the new fractionation gave a critical ratio here of 1.1. The critical

ratio for the general difference in power scores under the two variables was .56, while only those whose blood pressure was affected gave a critical ratio of the difference in power score of 1.3.

Actually, as the number of Ss analyzed is decreased, the criterion of significance in terms of the critical ratio rises. However, the above differences in critical ratios found were greater than the difference in the criteria of significance for such a decrease in the number of Ss employed in the two analyses.

Interpretation.—In every case in the above results, the average difference found between the two experimental conditions was in favor of the benzedrine sulfate condition. However, in no one case in the reasoning test itself was a difference found which was statistically reliable. Nevertheless, there seems, in general, to be a 'summation result' which indicates that 10 mg. of benzedrine sulfate, under the conditions of this experiment, does have an effect on syllogistic reasoning, mood, fatigue, systolic blood pressure, and heart rate, the effect being in general to increase these factors slightly (except fatigue, which is decreased).

Summary and Conclusions

Twenty Ss were given a syllogistic reasoning test at four different experimental sittings. At two of the sittings, the Ss were under the 'influence' of 10 mg. of benzedrine sulfate; the other two sittings involved a placebo capsule of lactose. Accuracy, time, and power (efficiency) scores were taken on the tests at each sitting. Records were also kept of mood ratings, fatigue ratings, blood pressure, and heart rate. Data were taken as the Ss' weight in kilograms, age in months, and Army Alpha examination scores.

Conclusions.—The following conclusions are all subject to the conditions and reservations of the experimental arrangement and procedure employed in this investigation.

1. The drug had no statistically significant effect on the reasoning scores of the Ss as a group, neither in terms of their accuracy, their speed, nor their efficiency alone. However,

all the changes were slightly in favor of the influence of benzedrine.

2. The benzedrine had only slightly greater effect on the women's reasoning scores than on the men's.

3. The Ss who were lighter in weight obtained a slight increase toward significance due to the benzedrine, which change was not as well exhibited in the heavier Ss.

4. Benzedrine had no statistically significant effect on the self ratings of the Ss as to mood and fatigue.

5. There was no statistically significant change in the rate of the heart beat of the Ss due to the effects of the drug.

6. There was a statistically significant rise in systolic blood pressure due to the effects of benzedrine. Also, the Ss who exhibited a rise in blood pressure due to the drug were the ones whose scores on the reasoning test were more appreciably improved by the benzedrine sulfate.

In general, all scores taken in the experiment were improved by benzedrine, but no one change in score was itself reliable (except blood pressure). However, this 'summation effect' gives indication that benzedrine sulfate does tend to increase the scores on a syllogistic reasoning test.

Suggestions for Further Investigation.—It is quite possible that the reason this investigation resulted in only a hint of an effect of benzedrine sulfate was that the dosage (10 mg.) of the drug was too small, or that the time interval between the administering of the drug and the taking of the experimental measures was too small, or both conditions were involved. A larger dose of the drug (15 or 20 mg.) could be used in such an investigation. However, if this is done, the Ss should be given a thorough physical examination to avoid any bad effects possible at such a dosage level. At this dosage (20 mg.) especially should a time interval of one hour or more be allowed for the drug to have full effect. It may be deemed desirable also to vary the dosage with the body weight of the Ss. Little is yet known about the dosage curve of benzedrine sulfate in terms of mental effect. This in itself would be considered a necessary and fruitful problem for investigation. Also, under similar conditions, an investigation should be

carried out employing other measures of higher mental function as the dependent variable. Also the relationship found here between rise in systolic blood pressure as such and the mental effect should be further investigated.

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STUDIES IN THE TRANSPOSITION OF LEARNING BY CHILDREN: IV. A PRELIMINARY STUDY OF PATTERNEDNESS IN DISCRIMINATION LEARNING

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Analysis of the response to transposed stimuli has been made by Spence (**8** and **9**) and by Jackson (**2** and **3**) in terms of conditioning. In their interpretation, choice (relative or absolute) in the critical trials is a function of two generalization effects. One of these is an excitatory tendency induced by the positive stimulus, and the other is an inhibitory tendency induced by the negative stimulus. Prediction of choice may be made by the algbraic summation of these two hypothetical generalization curves.

The type of analysis these investigators have made fails, according to Gestalt principles, to take cognizance of the important inter-relationships between the stimuli. The latter theory holds that what the subject learns cannot be broken up into two types of generalization, but that an irreducible pattern is perceived and learned. Razran (**4**, **5**, **6**, and **7**) has been calling our attention to the fact of conditioning to patterns. The present study is an attempt to determine whether there is a significant factor of 'patternedness' operating in the ordinary discrimination habit.

The design of this investigation is as follows: Two equated groups of child-subjects are put through a transposition experiment, one group under a condition favoring patterning and the other under a condition not favoring it. Patterning, that is spacial patterning, will be favored if the two training stimuli are presented simultaneously. If the stimuli are presented successively, patterning is less favored. If the results, upon transposition of the stimuli, are the same regardless of the difference in training, the Gestalt interpretation

will be discredited, at least in its application to this experiment. If different results are found for the two groups, then the 'conditioning' interpretation will require revision or extension.

Method, Procedure and Subjects

The photographic method for studying discrimination learning in children was used in this study. The stimuli were photographed on 35 mm film strips and these were projected on a screen in front of a group of subjects. Their responses were kept track of by requiring each child to drop numbered tickets into either of two (or three) ballot boxes. The numbers on the tickets were coördinated with the number of the trials. Further details of the method may be found elsewhere (1).

The three stimuli were squares, 5, 10 and 20 sixteenths of an inch respectively (linear dimension). These were placed diagonally on the square backgrounds which measured 50 sixteenths of an inch on a side. The largest stimulus covered but 16 percent of the surface of the background. The first two stimuli were used in the training and thus transposition was one step upward.

Since this was the first time that the group method had been used to study discrimination-learning, extra precautions were used in the preliminary to insure that all subjects perfectly understood what was expected of them. Forty trials were given to one of the two groups and sixty to the other. Preliminary training was given one day and regular training together with the critical trials were given the next. Each experimental period lasted about thirty minutes.

Two different sets of preliminary-training stimuli were used, one set was a pair of pen drawings of birds (an owl and a blue-jay), and the other set was two geometrical drawings (a triangle and a cross). The blue-jay and the cross were positive. Table 1

TABLE 1

Showing the Schedule of the Preliminary Practice Trials and the Manner in Which the Stimuli were Presented to the Two Groups

Simultaneous Group			Successive Group		
No. Trials	Type of Stimuli	Manner of Presentation	No. Trials	Type of Stimuli	Manner of Presentation
20	Birds	Simultaneous	10	Birds	Simultaneous
			20	Birds	Successive
20	Geom. Fig.	Simultaneous	10	Geom. Fig.	Simultaneous
			20	Geom. Fig.	Successive

shows the schedule of training for both groups. Since it appears to be easier to learn a discrimination habit with simultaneously presented stimuli, both groups were started in that manner. For the training in successively presented stimuli, three ballot boxes were used in the following manner: If the 'correct' stimulus appeared on the right side, they were to put the ticket in the ballot box on the right, if the 'correct' stimulus appeared on the left side they were to put the ticket in the left box; if the 'wrong' stimulus appeared on either side they were to put the ticket in the middle box. Since this procedure for the 'successive' group was somewhat more complex than that for the

'simultaneous' group, the preliminary was extended and it was presented to the subjects very slowly and carefully.

A question arises concerning how to equate trials in the two procedures. In the method of simultaneous presentation of stimuli, both stimuli are shown at one time. In the 'successive' method, only one stimulus is presented in one trial. Is one trial of the 'simultaneous' method to be regarded as equal in training value to two trials in the 'successive' method? The answer is probably no, but the trials under the two procedures are probably not equivalent. It was decided to make transposition tests at more than one stage of the training, so that the trend of the results will be clearly shown regardless of which is the true answer to the question. Table 2 shows the schedule of regular training trials and the transposition trials given to both groups of subjects.

The subjects were obtained in the William Penn School[1] of Mount Vernon, New York. All of grade 2–2 was used and a part of grade 2–1. These children ranged in age from 7 years and 2 months to 9 years and 10 months. They were divided into two groups equated for mental age. Each group had an average mental age of 6 years, 4 months. The equating was disturbed somewhat when one subject in each group was absent on the second day of experimentation. The final average mental ages were 6 years 5 months and 6 years 2 months for the 'successive' and 'simultaneous' groups respectively. Complete data were obtained for 12 children in each group.

TABLE 2*

SHOWING SCHEDULE OF THE REGULAR TRAINING TRIALS AND THE TRANSPOSITION TRIALS GIVEN TO BOTH GROUPS OF SUBJECTS

Simultaneous Group		Successive Group	
No. Training Trials	Transposition Trial No.	No. Training Trials	Transposition Trial No.
10		10	
20		40	
	1		1
20		40	
	2		2
40			3
	3		4
	4		5
	5		
	6		

* Note: The first 10 trials are given for general orientation to the new stimuli. If it is assumed that one trial with the 'simultaneous' procedure is equal to two trials with the successive procedure, then the results for transposition trials No. 1 may be compared. If the assumption is made that trials under the two procedures are equal, then transposition trial No. 2 (simultaneous) may be compared with transposition trial No. 1 (successive). It is hoped that the results will be sufficiently clear that interpretation of them will not depend on how to count the trials.

[1] We wish to take this occasion to express our appreciation to Dr. William H. Holmes, Superintendent of Schools in Mount Vernon, New York, and to Miss Martha Clarke, Principal of the William Penn School, for their courtesy in allowing the work to be done and for their fine coöperation in carrying out the investigation.

Results

Before referring to the results in the transposition trials, let us first examine the data from the preliminary and the regular training trials to see the extent to which the two groups appear to be equated in the performance of these tasks. Table 3 shows the difference between the average number of

TABLE 3

Showing the Number and Percentage of Errors in Both Preliminary and Regular Training for Each of the Subjects

Subjects	Simultaneous Group				Subjects	Successive Group			
	Preliminary		Regular			Preliminary		Regular	
	No. Errors	Percent Errors	No. Errors	Percent Errors		No. Errors	Percent Errors	No. Errors	Percent Errors
An.....	1	2.5	0	0.0	Br.....	2	3.3	0	0.0
Bo.....	2	5.0	2	2.2	Bv.....	0	0.0	1	1.1
Br.....	2	5.0	3	3.3	Ca.....	0	0.0	1	1.1
Fa.....	0	0.0	0	0.0	Co.....	1	1.6	4	4.4
Fu.....	4	10.0	0	0.0	Fa.....	1	1.6	1	1.1
Go.....	4	10.0	1	1.1	Go.....	0	0.0	0	0.0
Ma.....	1	2.5	0	0.0	Ke.....	3	5.0	4	4.4
Ro.....	4	10.0	1	1.1	Ki.....	1	1.6	1	1.1
Sa.....	1	2.5	1	1.1	Po.....	2	3.3	3	3.3
Si.....	2	5.0	0	0.0	Sa.....	2	3.3	2	2.2
Wh.....	1	2.5	0	0.0	Sc.....	3	5.0	2	2.2
Wo.....	0	0.0	4	4.4	Za.....	6	10.0	5	5.5
Average..	1.8	4.6	1.0	1.1		1.8	2.9	2.0	2.2
S.D.....	1.4		1.3			1.6		1.6	

errors made by the two groups in the preliminary to be 0.0. The critical ratio is also 0. This shows that there is not a real difference. The difference between the groups in the number of errors made in the regular training is 1.0. The critical ratio is 1.7, again showing that the difference is not reliable. Apparently it will be safe to assume that the differences between the results for the groups in the transposition trials will be due to the experimental factor in question.

Table 4 shows the choice made by each subject in each of several transposition trials. The difference is most striking. In the group trained to simultaneously-presented stimuli, every subject in every trial made the relative choice. On the other hand, only 27 percent of the 'successively' trained

TABLE 4

SHOWING THE CHOICE (RELATIVE AND ABSOLUTE) MADE IN ALL THE TRANSPOSITION TRIALS BY EACH SUBJECT

Subjects	Simultaneous Group						Subjects	Successive Group				
	Transposition Trial Number							Transposition Trial Number				
	1	2	3	4	5	6		1	2	3	4	5
	(After 30 trials)	(After 50 trials)	(After 90 trials)					(After 50 trials)	(After 90 trials)			
An.	R	R	R	R	R	R	Br.	A	A	A	A	A
Bo.	R	R	R	R	R	R	Bv.	A	A	A	A	A
Br.	R	R	R	R	R	R	Ca.	R	R	R	R	R
Fa.	R	R	R	R	R	R	Co.	R	R	R	R	R
Fu.	R	R	R	R	R	R	Fa.	A	A	A	A	A
Go.	R	R	R	R	R	R	Go.	R	R	R	R	A
Ma.	R	R	R	R	R	R	Ke.	R	A	A	A	A
Ro.	R	R	R	R	R	R	Ki.	A	A	A	A	A
Sa.	R	R	R	R	R	R	Po.	A	A	A	A	A
Si.	R	R	R	R	R	R	Sa.	A	A	A	A	A
Wh.	R	R	R	R	R	R	Sc.	A	A	A	A	A
Wo.	R	R	R	R	R	R	Za.	R	A	A	A	A
% Relative.	100	100	100	100	100	100		41.7	25	25	25	16.7
% Absolute	0	0	0	0	0	0		58.3	75	75	75	83.3

group made relative choices in five critical trials. The preponderence of 'absolute' choices for the latter group is great in all corresponding stages of training, regardless of how the trials are counted. After 50 trials of training with the 'successive' group, the percentage of relative choices is 41.7 (S.D. = 14.2), whereas, after either 30 or 50 trials the choices of the 'simultaneous' group were 100 percent relative. The difference in percent of relative choices made by the two groups, therefore, is 58.3 which is 4.1 times larger than the S.D. of the 'successive' group, a difference that is entirely reliable. If now the two groups be compared after 90 trials, it is found that the 'simultaneous' group still makes a 100 percent relative choice, whereas the 'successive' group has decreased its percent of relative choices to 22.9 (average of 4 trials) with a sigma of 6.1. The difference now becomes 77.1 percent or 12.6 times as large as the S.D. of the 'successive' group, indicating that the latter gives a very significantly

lower percent of relative choices than the 'simultaneous' group does.

Through the sequence of critical trials, in the case of the 'successive' group, the percent of absolute choices increases from 58.3 to 83.3. This is in line with the findings of an earlier study. In that investigation it was found, using the 'simultaneous' method, that the greater the amount of training the greater the probability of an absolute choice in the transposition trials (**2** and **3**).

The preponderence of absolute choices in the 'successive' group is about what would be expected if a great deal of practice had been given. It could be argued that under the condition of presenting a single stimulus at a time more attention is given to each stimulus and the training is thereby more effective. However, few readers probably will be content to accept so simple an account of the facts. It seems rather that a subject makes a somewhat different type of response to the two stimulus situation than to the single stimulus situation. It appears that the effect of training under the two conditions will not merely be a difference in magnitude in a single factor, such as practice. The twin-generalization hypothesis of Spence (**5**) as formulated by Jackson (**2** and **3**) would not fit both these sets of data unless a constant is placed in the formula to displace the 'transition points.'[2] The component represented mathematically by such a constant might very appropriately be called a 'patternedness' factor (P factor). The difference between the two sets of data is presumably due to the fact that in one case the subjects may see the stimuli as a pattern and in the other they probably do not. No attempt will be made in the present report to determine mathematically the magnitude of the P factor; however, in projected investigations it is hoped to make these determinations for both 2-stimuli and 3-stimuli training situations.

This finding and the interpretation of it do not discredit the twin-generalization of Spence. It is clear, however, that

[2] The term 'transition point' is discussed in a previous article (3). After a given degree of practice, upon transposition of the stimuli, the relative choice will be made; after more practice the 'transition point' is passed and the absolute choice will be made.

that theory requires an additional assumption be made in order to account for training under certain conditions cited above. The present writer feels this is an excellent example of how an adequate theory may be formulated, first by making highly specific assumptions (postulates), second by putting these to work for us and using them as far as we can, and to make additional assumptions when the occasion demands. This methodology will lead to a specific and a more nearly adequate theory than will vague theories which start from the top and usually stay safely in the clouds.

Summary

Two equated groups of 2nd grade children were given 90 trials of practice in a simple discrimination habit. To one group the stimuli were presented simultaneously and to the other they were presented successively about 5 seconds apart. Square patches measuring 5/16ths and 10/16ths of an inch set upon backgrounds 50/16ths of an inch were used as stimuli. Transposition trials, using stimuli of 10 and 20 sixteenths of an inch, were given after 30, 50, and 90 trials of practice.

The 'simultaneous' group made exclusively relative choises at all stages of practice. The 'successive' group made only 27 percent relative choice, and the number of relative choices decreased as the amount of practice increased.

These results are interpreted as indicating that there is an important 'patternedness' factor in discrimination-learning if the stimuli are presented simultaneously. It is suggested that the twin-generalization hypothesis of Spence, designed to account for the type of choice in the transposition experiment, will now require an additional assumption to represent this P factor.

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A SIMPLE KYMOGRAPH DESIGN

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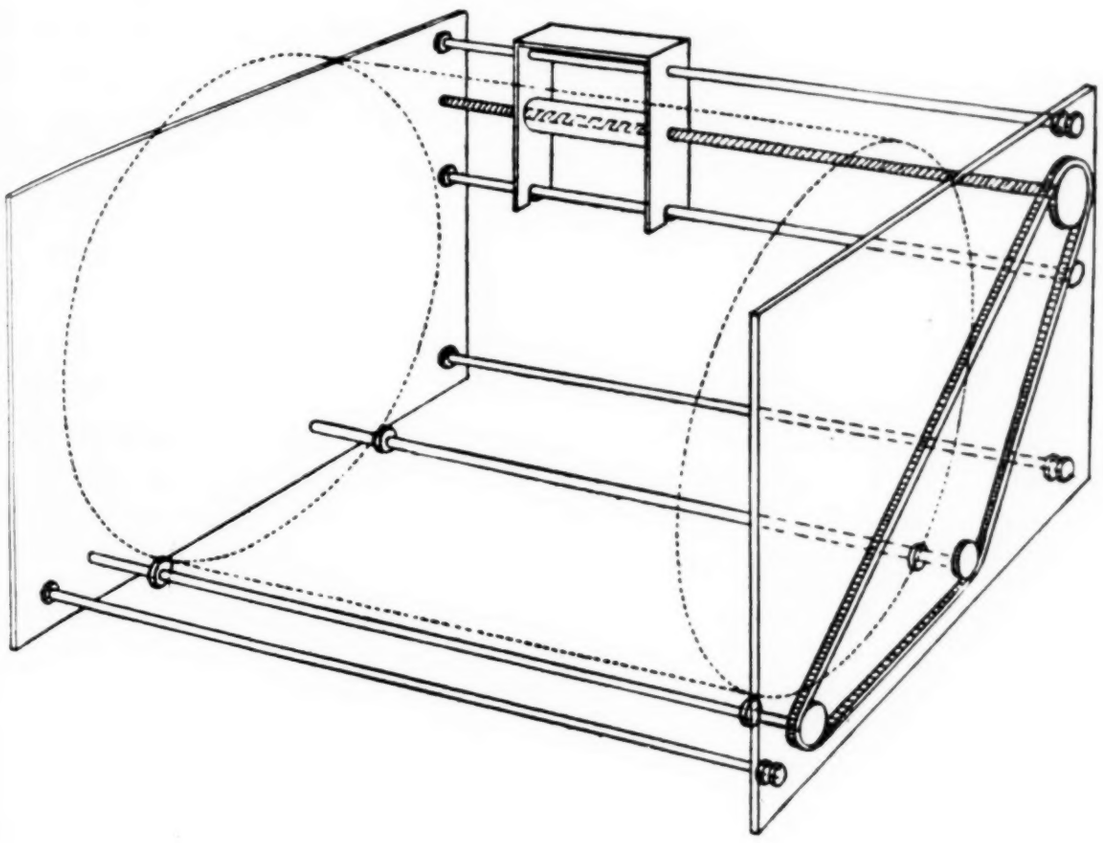
A great many investigations are hampered by the lack of funds for necessary apparatus. However, very often the cost of the latter can be reduced by suitable design. Kymographs and other time recording instruments are an appreciable source of expense in experimental psychology. In fact, most kymographs of ordinary design in common use cost more than $75, while an instrument suitable for most purposes can be built with about $4 worth of material and less than a day's labour.

A kymograph suitable for recording times greater than 1/20th seconds is shown in the accompanying figure. It may be described as being semi-kinematic in design. It is light in structure and a motor with an output of only 350 gm. cms, will provide adequate power to drive it. Most of the larger models of synchronous clock motors will supply this amount of power and a good quality model fitted with a gear box can be purchased for less than $6.00.

The diagram is practically self explanatory. The difficulty of centering a recording cylinder accurately is overcome by rotating it, not on a shaft through its own axis, but on four wheels rotating on two shafts parallel to its axis. The recording drum is marked in with dotted lines. A paint-can of good quality will be found to make a suitable recording drum. Such cans will have a seam along the length of the cylindrical surface parallel to the major axis, which must be smoothed over with solder, and two circumferential seams which mark the places where the bottom and the lip for the lid are pressed into position. The latter two seams form slight ridges on the outside of the cylinder and if these are made to rest against the wheels on which the cylinder rotates, they can be used to apply an additional constraint and prevent side motion. Of

course, this method of constraint results in an unnecessary loss of energy through friction, but the loss is small and saves the time involved in building more efficient forms of constraint.

Cans of the type under consideration are made with considerable accuracy. Any tendency for the drum to be elliptical in cross section will not result in any errors of recording provided that all recording is done in the same plane as the driving wheels and the axis of rotation. In such a case



only one pair of wheels should be used for driving purposes and the two pairs should function independently.

The two shafts which support the recording drum pass through plates at either end. These two plates are held apart and rigid relative to one another, by four rods, 3/16th inch in diameter. These rods pass through the plates and are bolted firmly on either side. Two of these are used to support a carriage which can move the whole of the length of the drum. The carriage is used to support recording pens and other devices, and when the instrument is in operation,

it can be moved continuously along the whole length of the drum by means of a thread cut on a rod, and geared to the driving wheels. The upper rod passes through two holes in the carriage, the lower one through two slits. This system means that the carriage is over constrained, but this permits a rather lighter design than would otherwise be possible. Pen recordings will make a spiral track. When a spiral recording track is used a correction must be made, but this will usually be small. For example, if the drum has a circumference of 45 cms, and the advance of the carriage per revolution is 1 cm, then the error per revolution will be 0.01111 cms, but it is probable that the thread which moves the carriage will not be sufficiently accurate to make this correction worth while.

A suitable pen for recording purposes is 'trylon' shaped with a slit along one of the edges. This was found to be much superior to the ordinary V-shaped pen. If heavily sized paper is used the usual water and glycerine ink will not be suitable owing to its tendency to dissolve into the size. In such a case a pigment dissolved in glycerine and alcohol should be used. Of course, recordings may always be made on a smoked surface.

I am much indebted to Mr. Fabergé of the Galton Laboratory, London, for his assistance in the design.

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